

# Antiquity

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## Editorial Notes

AT this time of year we always welcome an influx of new readers, many of whom no doubt will scan the contents of this number with special attention. Although we should naturally like this, our 25th volume, to excel, the present number has been composed in the same way as its predecessors, and may therefore be taken as typical, except that, quite fortuitously, it has fewer illustrations than usual. There may be quite a few readers whose chief or sole contact with the world of archaeology is through the medium of these pages. In turning them over they may be interested not only in what the writers of the articles say but in why they should say it at all. In other words they will want to know what sort of things archaeologists do, and what they think most important. Everyone knows that they excavate, but two of the Notes (on spear-throwers and Bidassoa 'ploughs') show that mere observation and record is also an important part of archaeology. It is quite easy to acquire this habit of seeing the past in the present—one which adds a new interest to travel and enables the interested layman to make his contribution.



Excavation is, of course, the prime business of archaeology—or perhaps one should say that it is not excavation simply but the prompt and adequate publication of his excavation. (We know that someone may retort that the writer's own first major excavation report has only appeared this year, 36 years afterwards! But for this there were special reasons entirely beyond his control). But there is also a vast amount to be done by mere field-work without excavation at all. This sort of field-work is hardly practised at all outside Britain and a few countries of Northwestern Europe. For instance there is a whole group of defensive linear earthworks, in the region where Hungary, Yugoslavia and Roumania meet, still awaiting field survey and description; see the writer's note in the *Geographical Journal* for last December (pp. 218–20). The model for all such work is Sir Cyril Fox's survey of Offa's Dyke and Watt's Dyke, published serially in *Archaeologia Cambrensis*. Admittedly that survey was facilitated by the existence of large scale Ordnance Maps, such as are not available in Central Europe; but their place could have been taken by air-photographs. During the inter-war decades no attempt was made to secure such by the moribund archaeologists of that region. Even a detailed verbal description of the course followed by the dykes, with rough diagrams, would have been better than nothing. That could have been composed only by walking along the dykes and in no other way. It would have had far more value than much that passes for excavation.

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One of the minor impediments to doing such field-work, both on the ground and from the air, is a quite excusable ignorance of the need. Those laymen who have the opportunity are apt to assume that such things have been done, that dykes have been walked and fully described, and that earthworks so large and well preserved must surely be well known. That assumption was actually made, in the instance mentioned above, by one who had such an opportunity. It was a perfectly natural assumption; but it may be said that it is safer always to assume the opposite. If there is any doubt, a letter to some archaeologist will generally elicit the needful information. So too those whose business it is to fly often see crop-marks and other things that are in fact unknown and which, if recorded, would very often rank as valuable discoveries; but how are they to know what is new and unrecorded and what is already well known? There is room for published guidance in such matters, indicating promising regions and the things to look for.



Defensive linear earthworks were chosen merely as an example. There are, of course, many other remains susceptible to field-work alone without excavation. Megalithic monuments are such. Much of the earlier study of long barrows and cairns, long and round, was vitiated because the archaeologists did not make adequate plans. They planned the burial-chamber (which they called a 'dolmen') but omitted the rest of the monument of which it was an integral part. One might just as profitably make a plan of a porch and omit the house. Even when the plan was complete it was often sadly inaccurate, and on far too small a scale, as the writer and Mr Hemp found when they began to survey some of the burial-caves of Mallorca and Provence. There is still no satisfactory plan of some of the classic monuments of those lands, and of many others such as Sardinia which abounds in megaliths. Without such plans all typological study is hamstrung; it cannot function without them; it is based on field-work and without field-work it is mere waste of time. In the past fantastic typological structures have been built by arm-chair students upon defective field-observation, only to be kicked over by the dirty boots of some ruthless field-archaeologist or excavator.



If the plans of the monuments are inaccurate or incomplete any conclusions based on them must necessarily be similarly defective. It is questionable, as a reviewer points out elsewhere in this number, whether the typological study of the ground-plans of burial-chambers can do much to solve problems of settlement and origin; and it is quite certain that it cannot do so unless the basic facts are correct.



The construction of castles in the air is not peculiar to archaeology. The same kind of thing took place in cartography. An accurate map can only be constructed by making measurements in the field, starting with a carefully measured base-line. The process of making a map of the world began well, though on rather too ambitious a scale, and culminated in Ptolemy's work. But during the Dark Ages his principles were forgotten, and though Ptolemy's work was used, every kind of distortion was introduced by a failure to work scientifically. Idrisi's 12th century maps are little better than caricatures of cartography. Not only did they differ from those of others, but they differ in each version of his own work. They are strictly comparable with the different philosophic systems which have emanated from comfortable professorial chairs ever



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since Plato. The nature of the universe cannot be discovered by introspection, nor is it possible by introspection to construct a map of the world or a chronological chart or a typological sequence that corresponds with reality. These tasks can only be performed by measurements and observations made in their respective fields by astronomers and physicists, surveyors and archaeologists—in other words by field-work.



It thus becomes plain why field-archaeology is of such fundamental importance. It is merely the archaeological equivalent of a procedure common to all other branches of science. To attempt archaeological work without doing field-work is exactly the same as to try and make a map of a country without going there, or even questioning those who have done so. Compare, for instance, the Hereford map of the World with that of Fra Mauro. Neither is satisfactory by modern standards, but Fra Mauro's, based upon the best information available in 1459—i.e. upon the field-work of travellers—is far nearer the truth than the other, based upon mere speculation.



All this argument may perhaps be met with the retort that it is unnecessary nowadays, that the case has already been decided, and that a dead horse needs no flogging. But if the horse is practically dead in this country, it is still alive and kicking in others. A constant stream of articles and books still comes from the archaeologically backward lands, full of pictures of potsherds and flints and devoid of plans and sections, or, if such are present, mere caricatures drawn from memory in an office or study. Such may be due to bad field-work or none at all. The impression still exists, and finds expression in print, that it is enough to live in a museum to be an archaeologist. Museums are of course of vital importance, and *ANTIQUITY* has often pleaded their cause; but they are not enough. They must be supplemented by field-work if they are not to degenerate into charnel-houses containing the desiccated corpses of potential archaeologists. The best museum curators of course realize this to the full, and would welcome opportunities for field-work. They are often still hampered by an obsolete and pernicious tradition which regards museums as bank-vaults rather than dynamos of research; and all of them are overworked. These remarks are directed not to them but against the system of ideas of which they are the victims. So long as that system prevails, and so long as museums are understaffed, archaeology must suffer from deficiency. When all this has been said it still remains true that there are some countries where museums abound and field-archaeology is non-existent. And by field-archaeology here is meant not excavation (which is of course a most important branch thereof), but all that exploration in the open air of the kind described above.

# The Origin of the British People: Archaeology and the Festival of Britain

by JACQUETTA HAWKES

A HUNDRED years ago, when Paxton's Crystal Palace went up among the elms of Hyde Park, it would never have entered the mind even of the Prince Consort that it should contain a section devoted to British Antiquity. The Great Exhibition was a glorification of the Present, of Progress, and its most conspicuous salute to the past took the form of a proliferation of Gothic ornament, and Pugin's gorgeous Medieval Court. The 1951 Festival of Britain exhibition on the South Bank will include a pavilion illustrating the *Origin of the British People* with material drawn almost entirely from the discoveries of British archaeology made since 1851. I do not think that this can be regarded as a tribute to the progress of our subject in any simple sense, rather it is a result of the development of the historical consciousness so characteristic of our time, and of which this progress itself forms a part. However, without further metaphysic, it remains a fact that one of the two official lines of entry into the South Bank Exhibition will be through a pavilion where many hundreds of thousands of people will find it difficult not to give at least a passing glance at a display of British prehistory.

My own association with the project came about in the most happily fortuitous way. I have long wanted to see a representative collection of Celtic Art, and it looked as though the Festival might provide an opportunity for staging one; with the blessing of the Council for British Archaeology I went to meet the appropriate official—who rather surprisingly proved to be the Director of Science. He thought Celtic Art would be splendid, but meanwhile it had just been decided to discard the mechanical grouping of exhibits employed in 1851 and so often since, and to give the South Bank show an organic form based on the historical interaction of Land and People. As I was there and interested, would I like to be the expert adviser on the Origin of the People? I agreed, and before long found myself something called the Convener of Section 12b. All other Conveners are members of the permanent staff, whereas since preparing the original 'treatment' I have acted only as an occasional consultant. The role of an anomalous Convener has proved most congenial.

It was understood from the first that one of my responsibilities would be to put the designers in touch with archaeological specialist of various kinds and with museum officials; this I have done, and many of them have been extraordinarily generous in the expert help they have given and the specimens they have lent. For myself, I hope I may have secured a position from which I can claim some credit for the show in general while denying all responsibility for any individual feature which may provoke criticism. That is my hope; it may turn out quite otherwise.

Before going further it should be made clear that Section 12b, the subject of this article, although it is by far the most considerable, is not the only archaeological contribution to the Festival. Apart from sites and excavations which may be on view to members of the special circular tours, there will be a smaller and differently designed version of the *Origin of the People* in the water-borne exhibition which is to visit our ports in *S.S. Campania*, while among the vast display of British scientific achievement



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in the Dome of Discovery our greatest overseas excavations will be included—from Mesopotamia, the Indus Valley, Palestine and Crete.

I have spoken of an organic form for the whole South Bank exhibition based on the growing relationship between a people and their land. It found expression in the original manifesto: 'The British contributions to civilization have as their ingredients the physical and mental make-up of the British people together with the varied and rich resources of the island which have stimulated the imagination and provided materials for subsequent invention and development'. With this theme the Exhibition was then conceived on an ideal plan with two main streams, one following the Land from its geological formation, the other the People from their historical beginnings, and both leading by way of agriculture, industry and so forth to the Dome of Discovery and all its modern marvels. It will be seen at once how in this scheme prehistory had its place at the beginning of the People stream, and how it was bound to be used to illustrate the building up of our racial stock. There was no other legitimate theoretical basis on which to work. That is why Section 12b seeks merely to demonstrate to the public that the British people is very mixed in origin, and how successive groups of invaders brought their own cultures and spiritual traditions yet rapidly assumed a distinctively British character once they were settled in the island. The assumption is that this blending has proved highly fertile and has contributed to the creative energies of the nation made manifest in the whole Exhibition.

If the underlying theme of the South Bank show largely dictated the theoretical basis of the *Origin of the People*, the physical peculiarities of the site had a considerable influence on its arrangement. They are very peculiar indeed. The approach has to be up a ramp which leads into two of the big brick arches below the Hungerford railway bridge. As visitors are conducted back into the prehistoric past electric trains will grind and roar overhead. Having been steered through the bowels of the bridge, they are to be led up another ramp into the pavilion itself, a fair-sized rectangular building which has had to be designed to allow circulation at two elevations, one round a broad gallery, the other at ground level. These physical limitations, stoutly reinforced by the customary shortage of funds, rendered impossible my original intention. This had been to take the public through a series of small halls where they would be engulfed in the prevailing atmosphere of various prehistoric phases—mesolithic life on the forest edge, a pastoral encampment and so on. Perhaps this defeat was just as well, particularly when one considers the number of people, pervasively 20th century people, who would have had to be engulfed at one time. Anyway, I saw it was hopeless, came away quietly and accepted a display which is more museum-like but which nevertheless has very considerable variety.

Before describing the work of making and assembling the exhibits, or attempting a critical assessment of our likely results, it will be useful to make a rapid tour of the entire section. The approach ramp, where no one can be expected to stop to look closely at any display, will run between steeply sloping sides as through a road cutting. In these slopes there are to be openings which will allow the passer-by to see antiquities lying as though newly uncovered in the earth—for example, a crouched Beaker burial, the Grunty Fen torque, a mosaic pavement and a hoard of coins. These discoveries, a caption will explain, provide some of the evidence on which the rest of the story has been founded. As a climax to this 'discovery' introduction, the first archway is to house a reconstruction of a large part of the Sutton Hoo ship, with the treasure partially uncovered, shown in such a way that the viewer must look down into it at an angle which allows the prow to jut up boldly.



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The next archway and the second ramp will have schematic maps demonstrating the isolation of Britain, the settlement of the island and the mingling together of the successive invaders. There are also to be large mural paintings which may not be art, certainly will not be archaeology, but will perhaps suggest in quite a powerful fashion the primeval land, the emergence of man and the invasions of Britain. On the threshold of the main pavilion greatly enlarged air photographs showing the marks left on our landscape by the prehistoric and early historic peoples are intended to call attention to field monuments as the second principal source of our knowledge.

The pavilion falls into six chronological divisions. The theory is that each of the periods so represented saw an addition to our racial stock with corresponding changes of culture. They can equally well be regarded as conventional chronological periods: the Mesolithic (Maglemosian), Neolithic, Bronze, Iron, Roman and pagan Saxon. Of these divisions, five are made up of similar components, three in number. A group, generally a family group, modelled at half scale and designed to show clothes, ornaments and a few utensils and weapons in a living context; a mounted display of material culture with a much larger range of typical possessions; a diorama about eight feet across with a scene showing a way of life characteristic of each period. The Iron Age division will be the largest, for in addition to the three standard components it will contain a carved model of a Celtic battle chariot with a pair of horses, warrior and charioteer. The display of material culture, too, will be extended to include a fair number of specimens of Celtic art.

Each of these five divisions will be given a background of a pattern representative of the period, but greatly enlarged. Thus the Neolithic will have a design taken from New Grange stone carvings, the Bronze Age from beaker ornament, the Saxon from the Franks' Casket. The effect promises to be very pleasing and not without interest.

Looked at as a series and not only singly, these exhibits should give a good impression of the historical development of material culture and also (through the dioramas) some idea of man's increasing control of his environment.

The sixth division is the Roman one, and this is differently constituted in order to suggest that here the chief impact of the invasion was cultural rather than racial. It will have a large screen with many features of Roman provincial civilization depicted on it, and with a single opening revealing the interior of a villa with furniture appropriate to the home of a prosperous Roman Briton.

On the way out from the pavilion there is a small, and, to my judgment, dubious exhibit where the conversion of England to Christianity is represented by stained glass roundels and a few examples of Christian Saxon art. The Viking and Norman onslaughts had to be shown outside the main pavilion and have therefore been illustrated by murals, the second conventionally but attractively carried out in the manner of the Bayeux tapestry.

The last room is intended for marshalling the public out of doors and passing them on to the next pavilions where the subsequent achievements of the British race, now constituted, are logically pursued! It was necessary, therefore, to treat this hall architecturally and keep it free from detailed exhibits. It has in fact been designed to restate in bold visual terms the simple theme that the modern British are descended from a fertile mingling of many stocks. Here for the first time the linguistic element of culture will be introduced by recordings of Welsh, Gaelic and local dialects, linked with trades and other activities proper to the various regions.

Before entering this final hall, the more earnest visitors can consult a reference section to discover the provenances of the field monuments and more important antiquities



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included in the exhibition. They may also, perhaps, be able to obtain some general guidance as to how to see something of early Britain for themselves.

This tour will have made it plain that while the intellectual content of the *Origin of the People* is of elementary simplicity, it has involved reconstructions on a scale never before attempted in this country. The amount of expert knowledge, of craftsmanship and artistic skill put into the task will be really immense, and an extraordinary variety of individuals and institutions has been pressed into service. They range from country craftsmen who have made single articles under expert guidance, to Cartiers, the famous jewellers, who have undertaken most of the personal ornaments, horse-trappings and fine metal work of all kinds, including even a coat of mail for the Saxon figure group.

The model maker responsible for the family groups is more accustomed to the production of charming figures for fashion displays, and it has been something of an effort for her to shape men who look as though they might be capable of head-hunting, women unused to cosmetics and lads and girls who have never known the playing fields of Eton or Roedean. However, she has toughened them up considerably, and one of my pleasantest memories is of going to her small workroom and there among a crowd of naked, half-scale early Britons discussing the problems of clothing them with two ladies learned in ancient weaving and dyeing, and a slightly bewildered dressmaker. As for the dioramas, we have, I believe, monopolized the skill of the very few men in this country who are proficient in this highly specialized art. Some of them are slowly assuming verisimilitude in a shed behind the Imperial Institute, while Iron Age and Saxon scenes are taking shape in the upper floor of a gaunt shed bordering one of the largest coal depots in South London. The Celtic chariot, warriors and horses are being carved somewhere close beside the King's Road. So it goes on.

This frenzy of manufacture has been accompanied by an equally energetic campaign of begging and borrowing. Many of the specimens in the first 'discovery' display are genuine antiquities and so, too, are most of those in the material culture exhibits in the main pavilion. The inability of our national museum to lend such things has made it necessary to turn to the Institute of Archaeology and a number of provincial museums. Care has, however, been taken never to seek to borrow specimens of outstanding importance, for it is understood that no museum should impoverish its own collections during 1951.

Needless to say there have been many problems, and I do not doubt that there will be much criticism. One of the greatest difficulties has been encountered in the field of physical anthropology, obviously fundamental in *The Origin of the People*. I started work still an untroubled believer in the naïve information which prevailed when I was at school. I thought one could say that the Neolithic invaders tended to be small, and perhaps dark, and might be represented by some of the small dark peoples of the Welsh mountains; that the Anglo-Saxons and Scandinavians were commonly fair and might have descendants in East Anglia and the Midlands; I even entertained ideas about the Beaker Folk. I now realize that this was very crude and that really either we are not descended from our forebears or our forebears had no features at all. However, I must not expose my irritability or lack of the scientific spirit. The fact remains we have had to allow our figures some personal appearance and so we have been obliged to fall back on certain naïve and crude ideas . . . .

Again, it has proved impossible not to swallow camels while straining at gnats. That is to say we have been meticulously accurate in many tiny details, while making bold guesses about such large matters as the cut and colour of clothes. The public will never know that the wheat is genuine emmer, or that the armband of the Anglo-Saxon

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woman is of exactly the correct weave and different from that of her dress, while the cloth is coloured only with ancient dyes. On the other hand they may well ask whether we really have any knowledge as to how the Neolithic peoples tailored their skin garments. I believe, however, we can claim to have used knowledge wherever it exists and resorted to guessing only where nothing is known.

The undertaking is certainly proving educative. It has been interesting to witness experts when confronted with some apparently simple question of reconstruction suddenly dumbfounded when they realize they have no idea of the answer. It has shown many of us how very little we know of prehistoric life from an everyday, human point of view, and has made us more aware of gratitude to those who—like Sir Cyril Fox and the chariot—have helped to lessen this ignorance. I believe it would be exceedingly good for archaeologists to do much more visualization of a practical kind. My husband seemed greatly to enjoy being made to work out the fastening of an Iron Age sword belt and the complete harnessing of a two-horse chariot. There have even been some discoveries, and I hope there may be more. For instance a precisely accurate copy of a Bronze Age jet necklace refused to lie flat until it was worn high round the neck like a choker, when it fitted perfectly.

One consideration has greatly strengthened our conviction that all this trouble and expense are worth while : it is the knowledge that much of what is being made will be useful long after 1951. When the pavilions are falling into decay and the success or failure of the Festival has been forgotten, many of these exhibits will still be as valuable as ever. Our museums have been notoriously lacking in good reconstructions to arouse the imagination of the ordinary public, and particularly of children. Here are many examples which if wisely allocated should help us to catch up with other countries in this branch of museum technique. We can be thankful to the Festival of Britain for putting money into archaeology and so enabling us to do a work for the public which I, for one, am convinced it is right and needful for us to do.



# Archaeological History : a review

by O. G. S. CRAWFORD

1. BRITISH ANTIQUITY. By T. D. Kendrick (Methuen, 21s.).
2. WILLIAM STUKELEY, AN 18TH CENTURY ANTIQUARY. By Stuart Piggott. (Oxford, 18s.).
3. A HUNDRED YEARS OF ARCHAEOLOGY. By Glyn E. Daniel. (Duckworth, 21s.).

THESE three books between them nearly cover the whole effective range of British archaeology, from the gropings and fumbings of medieval monks to the era of pollen analysis and radio-carbon. There is a gap of nearly a century, however, between the death of Stukeley in 1765 and the beginning of Dr Daniel's hundred years. That is the unfortunate but inevitable result of adopting the rigid yard-stick of a century for measuring what in fact is the growth of living knowledge.

Mr Kendrick shows very clearly how a single popular book, Geoffrey of Monmouth's *Historia Regum Britanniae*, held the field for four centuries as authentic history ; and that its influence was finally overcome not so much by direct opposition and argument (though there was plenty of such) as by the adoption of an objective method by a few scholars who went and looked at things for themselves—by field work, as we should now call it, using the term to cover every kind of observation and record from recumbent effigies to barrows. It was then that the 'confused, fabulous nonsense' and 'preposterous legends' that had accreted round the *Historia* were just crowded out ; being ignored they disappeared. That still holds good ; how much confused nonsense have not we of the present generation seen faded out by silence ! Where today are the once famous Children of the Sun, the Old Straight Trackers or the Phoenician tin-traders ? There is no room for these plausible hucksters in the crowded market-place of modern archaeology. But it should be noted that the new era was opened by another popular book, Camden's *Britannia* ; and that, if good books are to drive out bad, they must also be readable. (There have been many such published during the last 30 years).

In order to be fair in this criticism one should try to understand the mind of the medieval writer, but that is very difficult. We live in a world which doubts and questions everything ; they lived in one of faith, which accepted everything or nearly everything. When the facts, as they believed them to be, were contradictory, they simply gave up trying—*hanc ambiguitatem*, said Rous (1411-91), *non determino, totum relinquens Deo*. Confronted with similar ambiguities the Arab chroniclers were equally helpless : some say this and some say that, 'but God knows the truth best.' Such defeatism can only be overcome by an entirely different and critical approach.

The outstanding merit of Mr Kendrick's delightful book is its humanism. The author's sympathies are rather with the omnivorous Leland, so interested in everything he saw and in recording it that eventually he became lost in a forest of notes, than with Camden, the coldly efficient specialist who won through and wrote a classic. But Leland looked both 'hopefully forward into a new era of empirical research and practical survey and . . . with affection backward to the writing-desk of the medieval scholastic chronicler-antiquary.' That surely is the right humanist attitude, midway between the iconoclasm of the fanatic and the uncritical assimilation of the uncreative mind. Readers



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of ANTIQUITY know that Mr Kendrick too, for all his antiquarian lore, can also be as iconoclastic as any of us, though more gracefully. He has also a very nice sense of humour, of a vintage that sometimes only a connoisseur will savour.

Professor Piggott writes about one man, William Stukeley (1687-1765), so that, by good fortune, he almost carries on the story from the point where Mr Kendrick leaves it. But there is a gap, and in that gap is the great figure of John Aubrey. We wish that someone could now fill it. He would have a difficult task, for Aubrey has attracted many writers; but most of them have dealt with his purely literary work, and that, though highly important—he was the first English biographer, and wrote in a fine style—was not his only or even his chief claim to fame. He was the first field archaeologist; not the first who went and saw things for himself, for Leland and others had done that, but the first to study intensively the field monuments of a limited region and make plans of them.

Stukeley's best work was done in 1719-25 when he was in his middle thirties. Those years cover all his work at Avebury and Stonehenge and on the Wiltshire downs, and also the great antiquarian itineraries. It was a remarkable output, marked by originality and soundness of method. He observed the right things and recorded them accurately. He was many generations ahead of his times. 'He always tended to think in terms of maps and plans and drawings rather than of the written description, and on this is based the greatness of his work as a field archaeologist'. He even anticipated the field archaeologist's need for geological maps before the science of geology had been born. Like every true countryman he noticed the crop-marks which reveal ancient sites. His few excavations were mostly left unrecorded, but he was the first to draw an archaeological section (of a barrow). Why was it, then, that after 1725 he produced practically nothing of any value and much valueless speculation? Perhaps it was partly due to his isolation. 'The disadvantage of being a pioneer is that one has no colleagues engaged on the same researches who can effectively criticize one's work as it progresses.' There were other reasons, which the author analyses, some occupational and others perhaps psychological.

A generation ago Stukeley's name was mud even to such men as Haverfield, and there was some excuse, for Stukeley did not begin to publish his observations until long after they were made. The taking of holy orders was fatal to his objectivity of mind; he even went so far as to falsify his own earlier fieldwork in support of a theory. His later obsession with Druidical and other fantasies coloured all his published work, so that those who knew only these and not his original manuscripts could hardly be blamed for their erroneous judgment of his merits. By his scholarly examination of these documents Professor Piggott has given us at last a true portrait of the two Stukeleys—the keen and accurate field worker and the crazy theorist. But, even in his long period of decline, Stukeley was a sort of pioneer, for he was the first of that long series of country parsons whose occupational disease was uncontrolled antiquarian speculation.

Dr Glyn Daniel cannot of course be held responsible for the gap between his book and Professor Piggott's. That gap is wide enough to fill a separate book, for it includes the great touring books of Pennant and others, and Sir Richard Colt Hoare and his satellites. Amongst the latter was the Reverend John Skinner (1772-1839), incumbent of Camerton in Somerset, whose 98 volumes of journals, replete with plans and sketches, still repose unpublished in the British Museum. Haverfield used them for his V.C.H. articles, and others have dipped into them occasionally, but for the rest—and that means much—they are a rich unworked mine of raw material. Here is a task for some future



research-worker; and how much more profitable would be the outcome than most of the research devoted to nonentities! For Skinner, though by no means free from crazy theories, did record a vast deal of fact, much of it now gone for ever. And there are other such mines—the 100 volumes of William Cole (1714–82), the Cambridge antiquary (also in the British Museum), and the notebook of Thomas Leman (at Devizes). Sir Richard Colt Hoare himself, by far the best of all, deserves a book to himself, or at the least an *Archaeologia* article like that of Sir George Macdonald's on General Roy.

Dr Daniel's book is valuable because it contains much material and is well written in a modest sort of way, but it is unbalanced. That is probably partly due to the 'topical rather than chronological' treatment deliberately chosen; but it may be questioned whether the history of a subject *can* be treated otherwise than chronologically. Perhaps the field is too large to be covered by one man or in 341 pages. The main defect, however, is that the subject is not in fact archaeology but prehistory. The wheel has turned full cycle. Older books, such as that of Michaelis, almost or completely ignored prehistory; Dr Daniel has little to say about anything else. This limitation should at least have been stated in a sub-title; prehistorians will gain nothing by claiming too much for their own subject. What are we to think of a book by an Englishman professing to cover 100 years of archaeology but ignoring such great figures as Haverfield and Macdonald? And there are big omissions even of European classics, such as the 40 volumes of the Limes Commission, which had a great influence on the development of archaeological method everywhere. Throughout, the achievement of German archaeologists is ignored or undervalued—such works as Pauly-Wissowa, Forrer's *Reallexikon*, the *Corpus Inscriptionum*, Schuchhardt's *Atlas vorgeschichtlicher Befestigungen in Niedersachsen* (a fine early example of field archaeology). And what of Saxon and Viking archaeology? The great contributions of Norwegian archaeologists to British archaeology of the Viking Age—greater at any rate in volume than our own—are ignored. An outstanding leader like Wiegand did far more than might be gathered from the only passage (on p. 296) in which he is mentioned. Those who knew him and have heard of his achievements in many spheres would gain a totally inadequate impression thereof from this brief mention. It is as if, in a history of our own times, Churchill appeared merely as the protagonist of tanks in the first Great War. Nor is the rôle of archaeological inventories, an indispensable basis for research, adequately described; and we have produced a fine series in this country, both from government bodies like the Royal Commissions and from private individuals, such as Ellis Davies, Goddard, the Cunningtons and the authors of the Victoria County History articles. The index—an inadequate one, it is true—does not contain the names of Arne, Brögger, Bersu, Bøe, Haverfield, Macdonald, Munro, Lantier, Oelmann, Reinecke, Shetelig, Tallgren, Vouga. On the other hand a minor contribution to prehistoric archaeology receives 29 lines (pp. 188–9). There are gaps even in the history of the subject; an excellent chapter by Wiegand, Koepp and others in the *Handbuch für Altertums-wissenschaft* is not mentioned. The author does not appear to be cognisant of a bad and cumbersome but useful book by Gummel (*Forschungsgeschichte in Deutschland*, 1938).

The fact is that this is not the work of a field archaeologist but of an indoor student who, though cognisant of what has happened during recent decades and in sympathy with the modern outlook, does not appear to grasp its true significance. That is plainly revealed by a passage (p. 236) in which no distinction is made between excavations which were landmarks in the development of technique and others (at All Canning's Cross) which, though also a landmark in *discovery*, were lamentably deficient in that other respect. It is revealed also (on p. 290) by the citation of Ur rather than Erech as 'a

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noteworthy model of the whole modern technique of archaeology ' ; and by the excuses made for Égyptologists a few lines earlier. What was done by Petrie and Reisner could have been done by others, but they just were not good enough ; it had nothing to do with the climate. Nor is the alleged absence of stratification any excuse for deficiencies in technique, any more than its presence necessarily advances it, as French excavations everywhere show.

It will be apparent that, if these criticisms are valid, the book has many shortcomings, chiefly of omission and emphasis. It would be quite wrong, however, to dismiss it as valueless ; on the contrary it is in many ways a useful handbook, if read critically. It is, moreover, just because the book *is* a serious contribution to the subject that we have devoted so much space to it. There is no other book that covers the same ground, and in spite of its lapses one has the feeling that the author is trying to present the history of prehistoric archaeology in the right light, that is to say, to tell us, in terms of its history, what modern archaeology is. With more mud on his boots he might well have succeeded.



# The Gorsedd of the Bards of Britain

by IORWERTH C. PEATE

*National Museum of Wales*

PROFESSOR STUART PIGGOTT ends his admirable study of William Stukeley<sup>1</sup> with these words: 'It was a Druidic dream-world which has left an oddly tangible relic today in the Welsh Eisteddfod with its Gorsedd Circle, invented by Iolo Morganwg in response to romantic Welsh patriotism a century or so ago, but in the end to be derived from Stukeley . . .'. When I pointed out to Professor Piggott that, in this form, his statement was not strictly accurate, he suggested that a brief note on the Welsh Eisteddfod and Gorsedd would prove of value.

The Eisteddfod owes nothing to Stukeley: the word means 'a sitting' or 'a sitting in commission', or indeed 'an assembly' (the Psalmist's '*assembly* of the elders' is rendered in the Welsh Bible as '*eisteddfod*'). The word was used in the 15th and 16th centuries to denote a session of bards and singers meeting for the regulation of bardic conventions and procedure and for licensing bards, harpists and singers. In short, the medieval eisteddfod, in this sense, was a court held to regulate the many conventions and to grade and license the official classes of bards, harpists and singers. It was a type of court not unknown in other European countries.

The first assembly of this kind of which we have some knowledge was held in Cardigan Castle in 1176 under the patronage of the Lord Rhys. This meeting, like the modern eisteddfod, was proclaimed a year in advance, in Wales, Ireland, Scotland and England. It had two chief 'contests'—one for poets and the other for musicians who included harpists, players of the *crwth*, and pipers. The successful contestant was given a chair.

The next recorded eisteddfod (there must have been several in the intervening centuries) was held at Carmarthen in or about 1450 under the patronage of Gruffydd ap Nicholas. Here a small silver chair, a badge of office to be worn over the left shoulder, was offered for compiling a metric code for Welsh poetry. This was awarded to Dafydd ab Edmwnd. Cynfrig Bencerdd of Holywell was the winning harpist (no doubt invested with a silver harp) and Rhys Bwting of Prestatyn the winning singer; there is evidence that the badge of office of the chief singer was a silver 'tongue'. Another eisteddfod was held at Caerwys in Flintshire in 1523: of this we know little except that Tudur Aled made further modifications in bardic rules. Another eisteddfod held at Caerwys in 1568 is better documented. Held by the commission of Queen Elizabeth, the bards were to be licensed, and those found to be 'below standard' were to be prevented from roaming the country as vagrant bards. The silver harp (about six inches long) presented at this eisteddfod still exists. Professor Thomas Parry has drawn attention to a licence issued to Gruffydd Hiraethog at an earlier eisteddfod: the licence states that 'he is fully competent to have the degree of bardic disciple, and he is entitled to ask and take and receive gifts, of the good-will of the gentry and common folk, as is the right of all of that degree'.

There is little one can say about the eisteddfod in the 17th century, but there is evidence of many such meetings in the 18th century, although they were 'feeble affairs'.

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<sup>1</sup> Piggott, Stuart: *William Stukeley, an Eighteenth Century Antiquary* (Oxford, 1950), p. 188. [Reviewed on p. 10 of this number].

## ANTIQUITY

The turning point in the history of the eisteddfod came in 1789. In May of that year an eisteddfod was held at Corwen. It was organized by Thomas Jones of that town, and members of the public were admitted into the audience. This period in the history of the eisteddfod marks its beginning as a folk festival. The eisteddfod was now to be held more or less regularly and it was to function for many years under the auspices of Welsh societies set up in London (such as the Gwyneddigion Society and the Honourable Society of Cymmrodorion) and of similar societies active in Wales. The subjects for competition were now to be announced in advance, and in addition to the silver chair, medals and money prizes were to be offered. Through the influence of Lady Llanover and others, manual crafts were encouraged by competition and rewards. In 1858 a committee was set up to plan a National Eisteddfod to be held in alternate years in north and south Wales. In 1880 the National Eisteddfod Association was set up as a controlling body for this annual national festival: this was replaced in 1937 by the present National Eisteddfod Council.

This brief and inadequate sketch of the history of the eisteddfod will serve to show that the eisteddfod itself owes nothing to Stukeley; the eisteddfod is a well-established feature of the Welsh tradition, a medieval court transmogrified in the course of centuries into a popular festival the principal purpose of which is to safeguard and develop the Welsh language and the culture which it represents.

What then of the 'bards' in their 'druidic' costumes? The story is both interesting and amusing. Edward Williams, generally known by his bardic title Iolo Morganwg,—he was a Glamorgan man—was born in 1747. A stone-mason by trade, he was trained in the bardic tradition. In the early 1770's he went to London where he met a remarkable group of Welshmen who were interested in the language, literature and antiquities of Wales, and were indeed outstanding figures in the antiquarian movement. These men, disciples of such as Henry Rowlands and William Stukeley, were to spread the belief that Welsh bardism was descended from the Druids; this doctrine found in Iolo its most assiduous prophet. As Professor G. J. Williams, the greatest living authority on Iolo and his circle, has pointed out, Iolo declared that the bards of Glamorgan had secretly maintained the old druidic tradition throughout the ages, and produced manuscript evidence (of his own skilful forging) to prove his contention. The story of this remarkable man, a poet of outstanding genius, a French Revolutionary and maintainer of the 'rights of man', cannot be told here; it awaits the publication of Professor Williams's monumental biography. His forgeries were legion and most of them intended to glorify his native Glamorgan. However, about 1791 Iolo held his first druidic ceremony—the Gorsedd of the Bards of the Isle of Britain—on Primrose Hill in London, thus not only capturing the important London Welshmen of the day but obtaining publicity in such journals as *The Gentleman's Magazine*. The Gorsedd however was suspect in Wales for it became identified with the French Revolutionaries and their political sympathizers amongst whom Iolo was outstanding; in short the druidic movement appeared likely to die an early death.

Iolo's next step to save his movement revealed his genius. The Cambrian Society of Dyfed held an eisteddfod in Carmarthen in 1819. It was a three-day festival. *Iolo grafted his druidic movement on to this eisteddfod*. On the Saturday following the festival, he held a Gorsedd in the garden of the Ivy Bush Hotel, Carmarthen. The bardic circle and 'logan stone' were marked out with pebbles carried on his person, and the druids, bards, and ovates were invested with ribbons on their right arms. Before the end of the 19th century, the pebbles had grown into massive stone circles and the arm-ribbons into



## THE GORSEDD OF THE BARDS OF BRITAIN

the flowing white, green, and blue robes designed by Hubert von Herkomer ; the Gorsedd itself, instead of being an appendix to the eisteddfod, insinuated itself into the festival itself, claiming the right to crown the poet and chair the bard in full panoply and ceremony on two separate days, on both of which a Gorsedd circle meeting is held early in the morning.

Iolo introduced all the ' druidic ' refinements into his Gorsedd—the bardic script (*coelbren y beirdd*), the ' wooden book ', the mystic sign of three diverging lines representing the name of God, the division of the bardic year into *Alban Arthan*, *Alban Eilir*, *Alban Hevin* and *Alban Elved* (winter solstice, vernal equinox, summer solstice and autumn equinox), with all the spurious archaisms which his system necessitated. The bardic alphabet was a conventional simplification of ordinary characters adapted for cutting on wood, and one of Iolo's contemporaries, Dr William Owen Pughe, acknowledged himself to be the author of some of them.

This neo-druidic cult was nurtured and developed during the 19th century in Wales by some of the peculiar journals which were then in vogue, notably *The Cambrian Journal* which defended the druidic interpretation of Welsh history against all comers. Indeed one of its most vehemently denigratory reviews was directed against an article in the Welsh *Encyclopaedia Cambrensis* (1860) which threw doubt on the antiquity of ' druidism ' and (quite correctly) pronounced the triads of Dyvnwal Moelmud spurious.

Sir John Rhŷs's school of Welsh scholarship in the University of Oxford produced its greatest figure in Sir John Morris-Jones who in the closing years of the century published a series of papers exposing the Gorsedd myth. His work was taken up by Professor Griffith J. Williams whose exhaustive study of Iolo Morganwg and his Dafydd ap Gwilym forgeries was published in 1926. Subsequent work by the same scholar has succeeded in demolishing the whole fabric of false history set up by Iolo and amplified in many strange directions by his 19th-century disciples. No Welshman of culture today believes in the antiquity of the Gorsedd. But it still remains as the pageant (some of us would use the term circus) of the Eisteddfod. Most of the finest Welsh poets will have none of it, but a few distinguished figures (notably the present Archdruid) believe in its pageant value and wish to develop it in such a direction. Stukeley cannot claim the Welsh Eisteddfod, but the ' Gorsedd Circle ' is certainly his.

# Dental Evidence in Archaeology

by HUMPHREY HUMPHREYS

*University of Birmingham*

**T**EETH are the most indestructible of all organic structures. Even in life 98 per cent of their enamel is hard mineral matter and this dental durability has resulted in many extinct animals being known only by the fossil forms of their teeth. That is why the vocabulary of the vertebrate palaeontologist abounds in the names of species and orders ending in the Greek root 'odont'.

But long before the dawn of organised science it had not escaped the notice of savages that teeth are in fact the most immortal part of man as of any other animal. Of a body buried long enough in certain soils everything even the bones will crumble, leaving only the dentition deathless and indestructible. This has led to a tooth being regarded not only as itself immortal but as the vehicle of that immortality of the spirit which all simple folk desire. Savages prize a tooth as an emblem of vitality, and a savage chief or witch doctor often wears a necklace of them. In Africa when a king of Angola dies one of his teeth is put into a box with those of previous monarchs, and the possession of this box constitutes a credential for his successor. A whole complex of legends centre round the tooth of the Buddha enshrined at Kandy in Ceylon. And it is well known that the sacrifice of a front tooth often plays an important part in the initiation ceremonies observed by primitive people to mark the onset of puberty. These are but a few examples of the great mass of magical practice and fanciful folk-lore that centres on teeth. It arises from the fact that untutored minds cannot draw the sharp distinction that we do between animate and inanimate matter; any substance that appears to be incorruptible such as gold, horn or a tooth is held to possess the secret of life with which it can be made to part if suitably invoked or applied. From that universal belief springs the cult of gold by the Pharaohs of Egypt, the faith in the life-giving properties of a unicorn's horn held by the men of the Middle Ages and the modern Chinese, and the worship of Buddha's tooth.

But in a more practical way teeth can provide the field archaeologist with valuable data in his work. Our knowledge now enables us to draw a chart shewing the distribution of mammals during the ice ages of the Pleistocene and the inter-glacial epochs. Though it is rare for the excavator to find bones intact enough to identify the species, the teeth are always preserved and give clues to the date of the deposit which are of great value. Nor is their recognition always a matter for the expert. Any amateur can quickly learn to recognize the molars of the mammoth, the rhinoceros and the hippopotamus, and all three turn up frequently in English and European gravels. Indeed the former is so common compared with human remains that in the last ice-age mammoths may have outnumbered men by more than a hundred to one. Their tusks have been an article of commerce since Graeco-Roman times, and until modern plastics provided a satisfactory substitute for ivory, tusks from Siberia appeared regularly in the London ivory market in substantial quantities—several tons annually. They are seldom white but they served for red billiard balls and contributed an unrealized romance to the game. To separate sharply the teeth of the bison and the aurochs is difficult since they belong to the same family. But those of the hyaena, the cave bear, the cave lion, the reindeer and the wild horse are easily recognized.



## DENTAL EVIDENCE IN ARCHAEOLOGY

Human teeth provide evidence of greater value still. Providentially every single one of the 32 permanent and the 20 milk teeth of man differs from the rest and can be readily identified. They also differ from the teeth of the anthropoid apes, and of Neanderthal man, but those of the different varieties of *Homo sapiens*, ancient and modern, do not shew such specific variations as anatomists recognize in the bones. While different races exhibit variation of the size of the teeth there are no definite racial differences in their shape. This distinctness of human dental morphology is a god-send to the archaeologist and readers will remember the long debates on the teeth of Java, Piltdown and Peking man, and more recently on the dentition of the South African 'missing link', *Australopithecus*. But these debates concern details too technical for discussion here. Teeth are also useful as counters of heads. In 1937 the writer assisted in the excavation of the Iron Age fort on Bredon Hill. The camp had been stormed after the firing of the gates, the defenders cut down where they stood and the women and children carried off. No one had ever returned to bury the slain, and their bones were found where they had fallen, only a few inches under the turf. But they had been considerably scattered, presumably by badgers, wolves and foxes; like the foxes in Macaulay's vision of Naseby field, some had perished, and the anatomists had difficulty in estimating their numbers. By the simple process of counting the first left lower molars, less than an hour's work, it could be proved that not less than 54 men had died there, and this agreed with the tentative and much more laborious calculations based on the bones.

The sex cannot be told with certainty by human teeth alone though the jaws often reveal it. When the remains of individuals under the age of 16, or (if they have wisdom teeth) under the age of 21, are examined the age can be told often from a single tooth even if the jaws are not available, and always if there are three or four teeth to look at. This is because of the slow rate and regular rhythm of their calcification. Thus in the first permanent molar it begins at birth and is completed in the 10th year; in the second molar the cycle is six years later and carries us on to 16 years. Individual variations are normally only of a few months. To ascertain the exact age of a tooth is sometimes important. Everyone remembers the tale of the Princes in the Tower. In the reign of Charles II, two centuries after their disappearance, workmen uncovered two skeletons walled up under a Tower staircase, and on the supposition—it was nothing more—that these were the remains of the missing princes, they were deposited in an urn in Westminster Abbey where they have remained to this day. The late Professor William Wright obtained permission to open the urn and examine its contents. The dental evidence shewed the exact age of the skeletons which agreed with the known ages of the Princes, thus demonstrating their identity beyond any reasonable doubt. Here, as in other cases where the teeth are *in situ*, unerupted teeth, and teeth erupted but with roots incompletely calcified, could be made by X-rays to yield their evidence of age without damage to the specimen. Ages above 20 years can be assessed very roughly by the degree of wear on the tooth surfaces, but as this varies extensively with the diet many fallacies are involved.

Teeth, like bones, are sensitive to the recently developed fluoride test which reveals the relative ages of specimens found in the same deposit, though it does not enable us to compare one deposit with another. Thus although doubt still exists whether the ape-like mandible goes with the very human looking cranium found at Piltdown in 1912, it is now certain that they are of the same period.

The degree of dental caries in any skull is not a safe guide to its period, though the incidence of the disease—which now affects over 90 per cent of adults under the dietary conditions of western civilization—has varied a good deal from age to age. The figures

quoted are samples of investigations carried out on skulls whose period was well attested. In neolithic times in this country the percentage of individuals affected was 3 per cent, in the Roman period 25 per cent. In Saxon skulls it dropped to 15 per cent. Amongst Londoners buried at the beginning of the 18th century it had risen to 50 per cent, and since then it has doubled. The disease affects some animals: wild monkeys exhibit 2 per cent, captive ones 10 per cent of affected individuals, which is comparable with human figures. In Kaffirs living on primitive diet in the kraals, or Eskimo isolated from white men, the percentage is under 4 per cent. This rises rapidly in a few years to between 40 and 50 per cent when they eat the white man's food. Caries rarely occurs unless the diet contains some carbohydrate which can be rapidly broken down by fermentation in the mouth to an acid, a process which is sometimes completed in a few minutes. Such substances are the sugars, and starch, which either by natural or artificial processes has been converted into maltose. A remarkable example is the single skull of Rhodesian man whose mouth was ravaged by caries as much as that of any modern. We can conjecture either that he preferred his starchy food fermented, or that he cooked it. He was a late Neanderthaloid and certainly knew how to use fire.

Apart from caries there are some other relatively rare conditions which might conceivably yield evidence of value. Some dental abnormalities are inherited genetically as a Mendelian dominant. There is a rare defect of the enamel transmitted in this way, but the most famous example is the Hapsburg jaw passed in due course to the Spanish Bourbons and appearing in the royal portraits of five centuries. Some environmental conditions even if transient can leave permanent evidence in tooth structure. During the first 12 years of life, the critical time of tooth calcification, a vitamin or hormone deficiency, the absence or excess of certain minerals, will affect the process in bones or teeth and lead to faulty structure of both in the portions being calcified while the condition was operative. When it ceases to operate normal calcification is resumed and in bone—a vascular tissue regularly though slowly renewed—the faulty structure is repaired. Not so with the enamel and dentine of the teeth. Here the defective portion is never made good and remains as a permanent piece of evidence: and the damage can often be accurately dated. Thus it is not difficult to declare after examining the teeth that the individual during certain years lived on a water supply containing more than 4 parts per million of fluorine, or on a diet defective in Vitamin D, or that in a certain year some disease of a ductless gland producing a particular hormone began to afflict him. The examination has often to be made by microscopic section of the teeth. Archaeology has not yet needed to employ such evidence. But it is so omnivorous in its appetite for new techniques that it is rash to assume it never would. So it is perhaps well to put on record that it could be made available.



# Possible Magdalenian Survivals in Africa

by A. J. ARKELL

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THE interesting discovery of a spear-thrower in central Africa was made in 1937 by Mr J. S. Owen, then Assistant District Commissioner El Fasher, who heard of one being used in the Daju hills of Southern Darfur, and obtained two examples from Harun Yasin the man who introduced the spear-thrower to that area. It had not come into general use among the Darfur Daju ; and there was uncertainty as to its origin, when I was asked to find out from where it came.

Enquiry elicited that Harun Yasin had seen it used in Dar Sila by some strangers, and had copied it from them more as a curiosity than as an improved war weapon, for the occupation of this part of Africa has put a practical end to fighting with spears. Harun Yasin and a few of his friends in Southern Darfur had been using it without any great skill, but it is not used at all by the Daju of Dar Sila as far as I can discover, or by the Senyar or people of Fungoro in south-western Darfur who were suggested as possible users.

Prolonged enquiries were then made among the heterogeneous inhabitants of El Fasher town. Several people said they had seen the spear-thrower used by pagans in Dar Sila and near Raga in the western district of Equatoria province, but no one could tell me the name of a tribe who used it, until I found Mohamed Bugar, a retired soldier and native of Bagirmi, who said that it is regularly used in hunting and war by the Sarwa of Bagirmi, whose chief village is Chikina, and who are subject to Bagirmi. Nachtigal (*Sahara und Sudan* II, 672) describes the Sarwa as a pagan tribe in the east of Bagirmi. He states that their main villages lie on the north bank of the river Shari ; that many Bagirmi live mixed up with them, although the Sarwa people can be distinguished by their own dialect ; that they have been more or less subject to Bagirmi since about 1560 ; and that they are best known for the salt they manufacture.

Mohamed Bugar, who was about 60 in 1937, stated that in the days of his youth many of the people of Bagirmi had adopted the spear-thrower from the Sarwa and that it was used by footmen in the Bagirmi army. In his opinion the spear-thrower gives both increased distance and accuracy in spear throwing. Some spears which were made at El Fasher under his supervision for use with the spear-thrower have an overall length of 75 in. of which  $10\frac{3}{4}$  in. represent an iron head. The shaft, which is of wood, and thin in proportion to its length, is made of *inderab* (*Cordia abyssinica*) wood as usual, and the spear having no counterpoise is distinctly heavy in the head. Mohamed Bugar stated that 75 in. is the optimum length of spear for long distance throwing ; but that in war it was frequently more convenient to have some shorter spears for fighting at close quarters. In war about 10 spears are carried in a case (*turkash*) slung on the left arm. Four types of head are usually used in Bagirmi with the spear-thrower, a plain small blade for use in war, two types with multiple barbs for lion hunting, and a detachable head with a single barb on each side, to which a cord is attached, the other end of the cord being tied to the wrist. The last type is used for spearing hippopotami. When the spear-thrower is used for fishing, it is said to be used at close quarters only and its function then is merely to give increased accuracy of aim.

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The spear-thrower obtained by Mr Owen from Harun Yasīn (PLATE I, FIG. 2) consists of a piece of *ushar* (*Calotropis procera*) wood about 10 in. long,  $1\frac{1}{2}$  in. wide and  $1\frac{3}{4}$  in. deep. The top is hollowed to receive the butt end of the spear, and in the front edge is a groove in which lies the shaft. About 6 in. from the butt end a leather thong is attached, and on the underside of the spear-thrower a small step is cut in the wood in which the base of the palm of the right hand rests. There is a groove for the fore-finger in the butt end of the spear-thrower, and three grooves on the top of the spear-thrower on the right side near the butt end, in which lie the remaining digits of the right hand, to hold and steady the shaft of the spear before release. The thong which goes round the wrist and is taut when the spear is thrown, appears to increase the momentum.

No Bagirmi or Sarwa spear-throwers were obtainable while I was at El Fasher, and unfortunately Mohamed Bugar had seen the spear-thrower made by Harun Yasīn, before he made several examples, also of *ushar* wood and with a leather thong, but without the step for the base of the palm. In some of these he reproduced the grooves for the fingers, and in others not. In those which had the finger grooves he did not use them. His examples varied from no longer than that of Harun Yasīn to a maximum of  $26\frac{1}{2}$  in. long (or as he described it 'a cubit plus a span' long). In this longest example the leather thong was attached about 13 in. from the butt end.

Mohamed Bugar claimed to have been able to send a spear 100 yards with the spear-thrower, but being a fairly old man and very out of practice he failed in my presence to throw a spear with the thrower further than a good man could throw it without a spear-thrower. The spear-thrower with which he was most successful was about a foot long. He held it near the fore end with the thumb on one side and the fingers on the other, the spear being steadied in position by the tips of the thumb and of the third, fourth and fifth digits. The spear is thrown upwards at an angle of about 45 degrees to the ground, and falls almost vertically on to the mark.

In Harun Yasīn's example the spear-thrower does not lengthen the arm at all, while in those of Mohamed Bugar the arm is lengthened by from about 8 to 13 in.

In Bagirmi the spear-thrower is called *kafur*, and by the Daju it is called *merkaba* from its likeness to a shuttle or *jedaa* (Arabic for 'thrower').

All my enquiries failed to produce any other evidence of the use of spear-throwers in Darfur or Wadai, but it is interesting to note that while the Daju and other local Darfur tribes hold the throwing-spear at the point of balance for discharge, the Masalit of Western Darfur, who are skilful spear-throwers and use spears similar to the Bari types figured by Schweinfurth in *Artes Africanae*, hold their spears with the forefinger of the right hand on the butt end, and throw them upwards at the same angle as that used by Mohamed Bugar with the Sarwa spear-thrower.

This note was written in 1938 and given to the late Professor Henry Balfour, who was much interested in the discovery, and had promised to add a short note on the striking fact, to which he drew my attention, that the closest parallel to the Sarwa spear-throwers are those of the Eskimo, although he was at a loss to account for it. PLATE I, FIG. 1 shows two Eskimo examples from the Pitt-Rivers Museum, Oxford:—

- (1). P.R. 213. From Alaska (? between Icy Cape and Point Barrow) collected by the H.M.S. *Assistance* expedition, 1852-4.  $17\frac{3}{4}$  in. long.
- (2). P.R. 2003. From Holsteinberg, W. Greenland. Collected by the R.N. *North Star* expedition, 1852. 17 in. long.



PLATE I

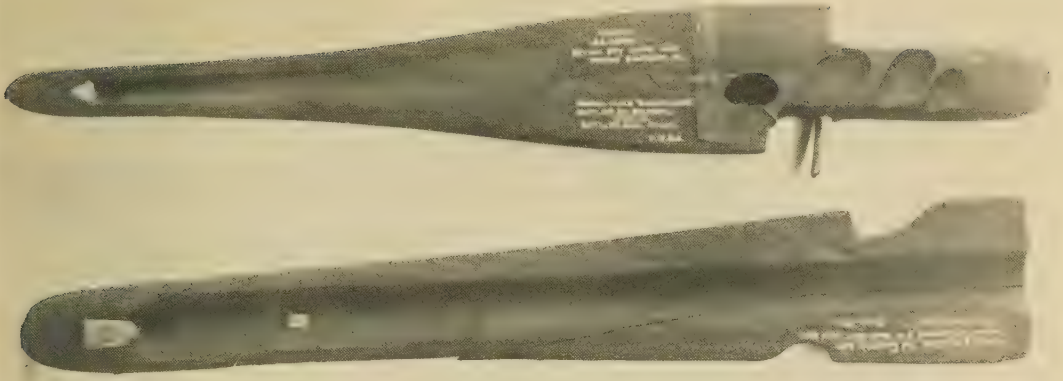


FIG. 1. TWO ESKIMO SPEAR-THROWERS  
By courtesy of the Curator of the Pitt-Rivers Museum, Oxford

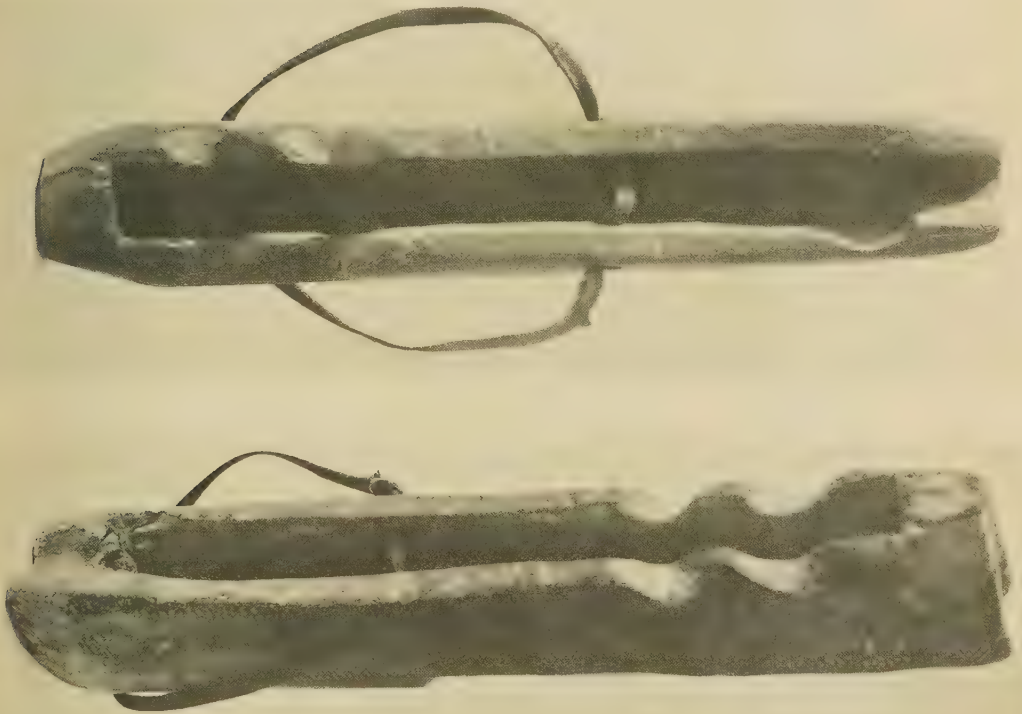


FIG. 2. THE SPEAR-THROWER USED IN DARFUR BY HARUN YASİN (TWO VIEWS)





## POSSIBLE MAGDALENIAN SURVIVALS IN AFRICA

The photographs, which were taken by Dr Meinhardt, are reproduced by kind permission of the Curator of the Pitt-Rivers Museum.

It will be noted that the Eskimo examples are not so short as the Sarwa spear-thrower used by Harun Yasīn but about the average length of those used by Mohamed Bugar, while they have a similar groove and socket to take the butt of the spear, and they also have notches made for the fingers.

An Eskimo using a spear-thrower is figured by Sollas, *Ancient Hunters* (3rd edition), FIG. 287, on p. 524. It is now fashionable to discount the remarkable parallels which Sollas drew between the cultures of the Magdalenian and the Eskimo. But the only possible explanation of the occurrence of spear-throwers apparently connected by their short length, groove and finger grips, as far apart as Greenland and Lake Chad that I can suggest is that they are peripheral survivals from a once more-widely distributed culture, of which the earliest known examples are represented by the Magdalenian.

Such a supposition may not be quite so impossible as it would seem at first sight, when it is realized that several other features of the Magdalenian culture seem to have survived in Negro Africa (that is notoriously conservative) at least until mesolithic times and in some cases until the present day.

The recent discovery of barbed bone spearheads, to which the closest parallels occur in the upper Magdalenian, at several neolithic or mesolithic sites in the Wadi Azaouak in the south-western Sahara and at Khartoum (see *ANTIQUITY*, 1947, pp. 172-81 and *Early Khartoum*, plates 46-49) is a case in point. At Early Khartoum also, as with the Magdalenians, the grinding and use of red ochre was important, ochre grinders and perhaps palettes having been found there that are indistinguishable from those found on Magdalenian sites. At Early Khartoum also were found a number of perforated stones similar to the Magdalenian example shown by Sollas on p. 534 (FIG. 297). No wooden objects survived at Early Khartoum or apparently at the Wadi Azaouak sites, but it is not impossible that wooden spear-throwers were used there as well as barbed bone spears and ochre grinders of Magdalenian type.

I am indebted to Mr T. K. Penniman, Curator of the Pitt-Rivers Museum, for drawing my attention to the fact that shaft-straighteners, which are used by the Eskimo and were used by the Magdalenians, being originally described as *batons de commandement* (compare Sollas, figs. 291 and 292) also have a fairly widespread distribution in Africa. There are in the Pitt-Rivers museum examples from Somaliland, the Latuka tribe (Upper Nile) and the Beli or Jur tribe from Rumbek, Bahr el ghazal, both the latter in the Anglo-Egyptian Sudan.

Perhaps even further evidence can be derived from expert examination of shells found on Magdalenian sites. The cowry shells from Magdalenian levels in French caves appear to be probably of Mediterranean provenance (e.g. Sollas op. cit., p. 588), but the Money Cowry recently recorded by J. W. Jackson in a Creswellian level at Pin Hole Cave, Derbyshire (*Proc. Liverpool Geol. Soc.*, xix, 1947, p. 178) suggests trade contracts direct or indirect with the Red Sea area in Magdalenian times, and if so, why not with the latitude of Khartoum and Lake Chad?

# Written and Unwritten Records\*

by the late STANLEY CASSON

ARCHAEOLOGY has, for its own good or ill, caught the popular mind. A generation ago the average person was quite unable to divest his mind of the idea that an archaeologist must have a long white beard, large spectacles and a pathetic enquiring look on his face: it was equally suspected that, when roused, he would burst into a paroxysm of rage if his views were doubted. There was, in fact, a good deal of truth in this popular conception, for in the first half of the 19th century what then passed as archaeology was largely a hobby reserved for the aged and the retired who, by virtue of curious minds and an inquiring nature, spent their leisure in probing the ancient sites of the past, and collecting ancient relics and 'curios'. Some less aged men, but nevertheless men of leisure, made very important contributions to knowledge, such as Boucher de Perthes, a French customs officer, who in the '50's was the first to identify palaeolithic implements; or Harrison of Ightham, a Kentish villager, who, with great insight identified, not long after Boucher de Perthes, a still earlier phase of the stone implement industry, the so-called Eolithic. But for the most part the elderly white-bearded men did in fact represent the activities of the student of Antiquity, both in England and elsewhere. Then, slowly, it was seen that there was a difference between the Archaeologist and the Antiquary: that the former was a student of one of the branches of humanist study which can serve, in the historic periods, to add enormously to historic knowledge, and in the prehistoric periods to formulate chronology and create a history recorded not by written records, but by archaeological facts. But the power of the written word, reinforced by the tremendous prestige associated with Holy Script, has given such kudos to what was written that records not consisting of words were considered by scholars as trash. Archaeologists were called, until quite recent years, 'collectors of pots and pans', men unworthy to rank with those who in the quiet of their studies had spent long hours emending and correcting the ancient literary records of literary men. To make an emendation in the text of a Greek manuscript was held by many to be a far greater achievement than to have recovered two hundred years of unrecorded history, even if the emendation was a pure invention of the emendator.

But that prejudice has largely died down. The literary scholars now realize the extent to which they must depend upon the archaeologist, whether they wish it or not. They have at last realized that the period during which written records, not procured by the archaeologists, are operative to explain the history of mankind barely exceeds two and a half thousand years, while the antiquity of man covers a period of 500,000 years. It is obvious to them that the recorded history of the human race covers but a short hour of its history. The archaeologists have helped them considerably by revealing more written records not suspected—the cuneiform of Babylonia and Sumer, the hieroglyphics of Egypt and elsewhere—which add another two thousand years to history as recorded in writing. But the tremendous prestige of a written document still holds, for we have barely passed out of that age when writing was the greatest invention ever made by man.

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\* This article originally appeared in the *Fortnightly Review* for February, 1937, and is reprinted here by kind permission of the Editor. Mr Casson, whose death on active service during the war came as a great shock to his friends, was a regular contributor to *ANTIQUITY*.



## WRITTEN AND UNWRITTEN RECORDS

The world has been literate, after all, only in fits and starts. Mesopotamia was the home of the great invention, and Mesopotamia alone remained constantly and continuously literate from about 3000 B.C. to the Middle Ages, a record which has nowhere else been beaten.

If proof of this somewhat startling assertion is needed, it can be given in two ways. In Mesopotamia, under Sumerian rule, under Babylonian, Assyrian, Persian, and Sasanian, the Kings frequently made for themselves great public monuments. Such monuments are usually inscribed, sometimes with long and detailed statements of the achievements of the ruler and the extent of his conquests. Such is the great monument of Darius at Behistun, carved in the rock, inscribed with an account of the Persian Empire. Assyrian inscriptions are perhaps even more widespread and more specific. Such monuments were made to endure for all time—which for the most part they have—and they were designed to impress the passing traveller, the caravan, and the citizen. They presuppose that he could read them.

Secondly, throughout the same periods every merchant and every man who had even a small amount of property, possessed also a seal-stone with which he could seal the documents of daily business, the cuneiform tablets which then took the place of ledger and account books. The number of Sumerian, Babylonian, and Persian seal-stones which have been found is enormous. Many of them are actually inscribed with the owner's name. They also presuppose a widespread knowledge of reading and writing.

The Persians who succeeded the Sasanians and lasted until the Mongol. invasions of the 13th century A.D. were, perhaps, not quite so literate as their predecessors. But the widespread interest in poetry and the large number of Persian writings that have come down to us testify that the deep-rooted knowledge of reading and writing in Mesopotamia and Persia was not finally eradicated until the Mongol destructions. Even then much survived after that calamity, though, for the most part, the ordinary man now became illiterate.

In comparison Greece and Rome produced a civilization which was literate only for some 1,200 years. With the end of the Roman world literacy, in the sense that the average citizen could read and write, ceased for several hundred years. In Greece as in the Roman Empire, everyone had these achievements except slaves. Even the Romano-British working man was literate. You will still see written on tiles found in London and elsewhere, the odd remarks and witticisms which the workmen scratched in their luncheon hour. But few workmen were literate from the fall of Rome to the 19th century in any European land. We had to start all over again with the tiresome business. And every time that man starts anew to read and write he marvels anew, for the glamour of the invention is still strong upon him. It is so very recent a discovery, even if we date it back to the time of the early Sumerians, compared with the total history of Man.

That, I think, largely accounts for the prejudice which surrounds anything recorded in handwriting or print. And that prejudice is reinforced by the overwhelming power given by religion to its testaments. If the Bible says so then it must be so, is a dogma belonging not merely to a mediaeval age or even to a reformed Puritan era, but to the days in which we live. It is recorded, and therefore it has a power and truth not permitted in other mediums of recording.

But there is a reaction and, in its way, the reaction is almost as dangerous to the study as the original animus against purely archaeological facts. On the one hand, the older generation of scholars said that 'If your story is based solely on archaeological knowledge, its worth is infinitesimal in comparison with a story recorded in written script'. On the other, the enthusiasts will state that 'Since written records are often

tendentious and erroneous, they are nothing like so valuable as the certain facts produced by excavation'.

Between these two extremes the unhappy archaeologist has to make his cautious way. Some archaeologists, through excess of caution, merely tabulate the bare facts of their discoveries, cataloguing them like a seedsman or a grocer, and leave the student to make what he can of them, letting the literary man draw his conclusions if he likes. Others, casting caution to the winds, make wide and sometimes rash inferences from evidence which will hardly permit it, in order that they can justify their claim to be as good as the professed historians.

The wiser course is to realize that neither way can stand alone. An archaeological report that is a mere catalogue is an incomplete report. Conclusions must be given by every excavator. But the conclusions must adhere with close and absolute fidelity to the listed facts. You cannot go outside your terms of reference—or you should not.

And so, in the long run, the facts of archaeological discovery and those recorded by history and literature must be taken together when they exist together; the conflict must be dealt with, if there is a conflict. If recorded history states one thing and archaeological facts indicate another there must either be a reconciliation between them or one set of facts must be preferred to the other. The glamour that surrounds written record must be cleared away by the wind of impartial scrutiny. If a historical writer says that at a certain place at a certain time a certain state of affairs existed, and archaeological excavation proves conclusively that it did not, then we must accept our historian as wrong and conclude that, even if he be Herodotus or Thucydides or Gibbon or Mommsen, he wrote in error. Such a situation is constantly arising in archaeological research.

There are many instances which I have not the space to mention here. But I might quote, for example, the way in which archaeological research at Sparta has revealed that the Spartans, so far from being the inartistic, puritanical, militaristic luxury-hating people of Greek tradition, were, in fact, at least in the 7th and early 6th century B.C., one of the most luxurious and artistic peoples of Greece. The excavations showed that there was in this early period an almost oriental love of music and adornment, a wide interest in poetry and music and little or nothing of puritanism. Later there came the puritanical revolution which made Sparta a byword for uncultured simplicity throughout history. But the historians had forgotten that early rapture of art and culture, for it happened many generations before they wrote. I might quote also the strange enigma of the Phoenicians who are supposed still by many to have colonized Britain and to have brought eastern culture to our islands. History tells us of them, but archaeology not only can produce no single object of their importation, but it states quite firmly that, if indeed any Phoenicians did come here, we have no grounds at all on which to base our belief in that supposition. True, the archaeological refutation of the historians is largely based on negative evidence. But it is negative evidence of a peculiarly cogent kind. For, if after nearly a century of excavation in our island, no trace of Phoenician importations and commercial activity has been found we can with tolerable safety suggest that there was none to find.

Here are two myths of the historians now dissipated. There are even certain myths of the emendator which archaeology has banished! In the *Ethics* of Aristotle occurs a strange passage referring to 'man, who won a victory at Olympia in the games'. It is not 'the man' or 'a man' but just 'man'. As such it long baffled the textual critics. One inserted 'the', without textual authority, while another, more ingenious, suggested that a proper name had dropped out of the text and someone had put in brackets, simply 'man', in the sense of 'Another'. Then, when a papyrus found in Egypt was at last



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deciphered and published in the second volume of that great work of erudition, the *Oxyrhyncus Papyri*, we were given a list of Olympian victors from a forgotten treatise on the Games. In it appeared among the winners a Greek with the singular and unique name Anthropos. Here was our enigmatic winner whose name was, simply enough, Mr Man. That made the sentence read as sense and now the latest texts print the word previously printed as 'anthropos' as 'Anthropos'. A small point indeed, but one where the archaeologist held the key, for the papyrus which cleared the matter up came from an excavation.

The facts recorded by an ancient historian are facts recorded by a man who lived at the time of the events he records, at least in the case of Thucydides and similar writers. That gives him strength as a contemporary witness. But the facts of archaeological research may also be contemporary, and they are every bit as valuable as witnesses if they can be interpreted.

In recent years the agreement between the historian and the archaeologist has been much reinforced. Each year we find the newly discovered facts confirming anciently recorded statements, in those periods where archaeology and written record cover the same ground. And, naturally enough, it is the Greek period where the highest standard of complementary agreement is to be found, for Greek historians are, on the whole, more accurate observers than historians of other ages. To take a small example. We have all heard of ostracism. The term literally means 'the process of the potsherd'. A referendum was taken of the Democracy at Athens whenever there was a political impasse, and every citizen was asked to scratch on any chance potsherd that he could pick up, the name of the politician who should, in his opinion, leave the country for the country's good. The size of modern democracies makes this salutary process, today, alas, impracticable. As schoolboys we had all heard the story of Aristides the Just, and his ostracism. But, somehow, we never quite believed that the process occurred literally as indicated by its name. We imagined some sort of public vote by show of hands. Not a bit of it. The excavators of the Athenian market place, who have now been digging for several years, have accumulated an almost embarrassing quantity of broken fragments of pot, roughly inscribed with the names of leading politicians of Athens in 5th century B.C. Among them occurs the name of Aristides with considerable frequency. All the names of politicians recorded by Thucydides and Aristotle (in his *Athenian Constitution*) occur on these sherds, and others not hitherto referred to by the historians. Almost a hundred in all have been found, thrown away after the grim process was concluded. A complete history of Athenian Ostracism can be written from their evidence. The unknown names can be roughly dated by the form of their letters. History is thus illustrated, vindicated and enlarged. 'The pot and pan hunter' can no longer be jeered at by the historians.

We all remember at school reading through the long and thorny passages of Thucydides in which he describes those exciting events at Pylos and Sphacteria, when the relatively amateur Athenian soldiery succeeded by a brilliant coup in forcing a detachment of the finest soldiers of Sparta to lay down their arms and surrender on a rocky island off the west coast of the Peloponnese. Even in a drab and dusty schoolroom we could glimpse the excitement that must have run so high in Athens at the time. Many of us since have seen the lovely Victory of Paeonios in the Museum at Olympia which was made to celebrate the event. Now in the same Athenian excavations has been found a bent and battered bronze shield on which, after cleaning it, the excavators found roughly punched the inscription '*Dedicated by the Athenians: from the Spartans at Pylos*'. When the captives and their arms were brought in triumph to Athens, their shields were

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hung up in one of the public buildings. Somehow one of the shields a century later was dropped into a cistern which was filled up. There the excavators found it. The events it commemorates live in the lucid pages of Thucydides. There you find the story of how Cleon the demagogue swore to conclude the siege of the Spartans in twenty days. The people of Athens, who almost took him as a joke, gave him permission to try his hand and, to the surprise of all Greece, he succeeded. The four hundred odd Spartans were brought back, and one of the soldiers of Cleon punched this rough inscription on the shield of his captive. What better illustration to Thucydides is there than this ?

Then what a mass of information archaeology has provided for us about the battle of Marathon. There is the burial mound of the Athenians on the battle-field. Excavated many years ago, it yielded the humble oil-pots and vases which, after the battle in 490, the relatives of the dead had placed beside the bodies. Arrowheads from the battlefield were also found buried with them. But the recent excavations at Athens have provided a more precise documentation still. A fragmentary inscription was found in the Athenian market place which preserved the half of two four-line verses. It was found to belong to another fragment, long known, but unidentified. The whole gave two verses which commemorated the dead of the battle, and the verses were of a very high literary order. Research showed that, in the pages of an obscure writer, it was stated that in the competition, soon after the battle, held to select the official inscription for the official war memorial of the event, Simonides, the famous poet, won the prize ; while Aeschylus, the dramatist, came second. Rumour had it that Aeschylus, a native-born Athenian, was deeply offended that the prize was given to Simonides, who was a foreigner. On the stone found, the first dedication can indisputably be assigned to Simonides on grounds of style. The second might well be by Aeschylus : but it would be strange if the second prize-winner also inscribed his verse on the war-memorial. Yet he certainly did so, for a closer scrutiny of the stone showed that the second verse was added after the erection of the monument. We can thus safely reconstruct what happened. After the monument was up, perhaps only a month or so later, local support of Aeschylus and his sympathisers persuaded the authorities to add the verse of the poet who only came second, so as not to wound his susceptibilities. All were then satisfied. A very Athenian compromise.

But the question remained how it happened that both these verses had not survived in the various anthologies of epitaphs and epigrams which have come down to us, among which the verses of Simonides are numerous, for he was one of the loveliest of all Greek poets. The explanation is this : ten years after the victory of Marathon, the revengeful Persians came again and captured Athens and laid it waste. One of the first objects of their destruction would naturally be all those monuments which commemorated the battle of Marathon. And so they broke this stone and knocked it over. Then it got buried in the debris until the excavators found it. That explains how it came to be omitted from the anthologies, for they were not compiled until well after the second invasion of the Persians, and by that time the broken memorial was buried.

Here again is a chapter of history, perhaps more literary than political, recovered by the spade, and two admirable poems added to the anthologies of Greek poetry. Could one expect more from the excavator ?

In other fields where the written records are weak or confused much more depends on archaeological discovery. The recent campaign in this island to investigate the state of Britain in the periods just before and after the Roman conquest was a well-thought-out attempt to provide historical information where the written history fails us. Before Julius Caesar made his first raid on our shores the native Britons lived in no little luxury



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in well-organized petty princedoms. Greek historians and geographers have left us some few accounts of our ancestors, but they are sketchy in the extreme. After the Romans left us, Britain faded away into a Dark Age that was perhaps darker than any other in history. We know almost nothing of what happened between A.D. 500 and 700, and the available historians are as illiterate as they are unreliable.

What we want to know is whether the pre-Roman Britons were really savages painted with woad, as Mrs Markham's *History of England* would have us believe, or whether they were a chivalrous and aristocratic society, highly organized and deeply influenced by foreign cultures, as their remains would indicate. Excavations at sites like Colchester and Maiden Castle have revealed the truth. The pre-Roman Britons may not have lived like Romans, but they certainly did not live like Hottentots. The post-Roman Britons, on the other hand, appear to have lived in a revived barbaric splendour in which traditions of their pre-Roman ancestors, long dormant, had begun to revive. These are new chapters of history which consist of archaeological enlargement on very bare themes recorded imperfectly in written records.

The increased popularity of archaeology is evident on all sides. The number of people who attended the purely archaeological exhibition held at Burlington House, to illustrate the activities of the British School at Athens, far exceeded the expectations of the organizers. Provincial archaeological societies are nowadays prosperous as they have never been before, and hosts of young enthusiasts are ready to help all excavators. There are the professional archaeologists working hand in hand with the amateurs. But neither must expect too much. The professionals must not expect that the amateur will be content with mere catalogues and reports written in pure jargon, and the amateurs must not expect that the professionals are prepared to launch vast assumptions which their evidence will not sustain. The perpetual cry of 'Show more boldness and more imagination' is a demand which must not and should not be made. It is like asking a doctor to give a precise diagnosis when he knows that he must suspend judgment. No one is more welcome into the professional archaeological fold than the judicious and well-trained amateur, and no one is more unwelcome than the irresponsible enthusiast who brings the whole study into disrepute. The study of archaeology is illuminated by the contributions of pure amateurs. Schliemann, who laid the foundations of prehistoric Greek archaeology, was in all senses an untrained amateur. Almost the whole of palaeolithic archaeology was first organized by amateurs. Central American archaeology was mainly begun by Maudsley, himself an amateur, and today much first-rate work is in amateur hands. But when the amateur seeks to step out ahead of the professionals and play a lone hand he will, in nine cases out of ten, lose the game with disaster. For in no other study is international co-operation so absolutely essential as in archaeology. Every step you take must be based on comparisons and research which concern the work of others. A historian, like Gibbon, can play a magnificent lone hand with prodigious success, but only because he can draw from the researches of others. Even so, his main outlook may prove to be wrong. Few Byzantine historians today would support Gibbon's general view as to the character of the Byzantine empire. Gibbon worked too much in the study and too little in collaboration with others, and the material remains of Byzantium meant nothing to him. Today the archaeologist and student of art can re-write his history in another vein. So that even the greatest historians can miscalculate if their work is not checked by the discoveries of archaeology.

## Notes and News

### THE RECENT EXCAVATION OF PRIMITIVE HUTS ON THE PALATINE

Dr Puglisi's all too short account of the recent excavations on the Palatine<sup>1</sup> will be of interest to many and deserves to be brought into closer relation with other discoveries in Rome. The huts found in the lowest level, as Dr Puglisi points out, belong to a settlement connected with the well-known cemetery in the Roman Forum. The strata connected with this settlement yielded pottery and other remains reaching down to about 550 B.C. This date must be associated with the expansion of the settlement to include the villages on other hills. Archaeologically this development is marked by the construction of the agger and fosse crossing the ridges of the Esquiline, Viminal and Quirinal. This agger was in existence in 378 B.C. before the building of the Republican wall, which follows the same line in this part of the circuit. A closer date is given by the three graves of *circa* 600 B.C. found under the undisturbed bank in the grounds of the old Villa Spithoever on the Quirinal.<sup>2</sup> This and other evidence points to a date in the 6th century, a date in full accord with the traditional ascription of the vallum to Servius Tullius (*Livy*, I, 44, 3). This agger enclosing a considerable area does not stand alone. At Veii a similar earthen bank bounds the city on the northwest, the only side where it lacks natural defences. The bank was later strengthened with a wall, but the original earthen agger must be as old as the 6th century. At Ardea the great earthen bank and ditch run from valley to valley, cutting off the area of the city, and this defence has been shewn to be older than a gateway attributed to the middle of the 5th century.

The dating of the early remains in the Roman Forum is difficult to establish with precision. Two of the graves, G<sup>3</sup> and AA<sup>4</sup>, contained proto-Corinthian lecythoi of the 7th century; others such as M<sup>5</sup>, yielded numerous bronzes which carry the closing date down to about the end of the century. The next stage is recorded in the late 6th and early 5th century when there is record of several dedications in and around the forum. The accepted date of the temple of Juppiter Capitolinus was 501 B.C. and from that beginning the series runs on to the Temple of Semo Sancus in 466 B.C.<sup>6</sup> Furthermore Roman tradition ascribed to a number of these buildings, including the Temple of Juppiter and the Cloaca Maxima, an origin in the later regal period, in the latter part of the 6th century.<sup>7</sup> The Cloaca Maxima is of particular importance in this connection, for without this drainage the valley would have remained a swamp unfit for use as a market

<sup>1</sup> ANTIQUITY XXIV, 119. [The substantive report has now appeared; see p. 36. Ed.]

<sup>2</sup> G. Saeftlund, *Le Mura di Roma repubblicana*, 154 and 231. The question whether the bank at this point was the earliest agger or an extension (*Monumenti Antichi*, xv, 249, n. 1) is not pertinent in the present context, as the graves lay within the city and would not have been placed in such a position after the construction of the agger.

<sup>3</sup> *Notizie degli Scavi*, 1903, p. 389.

<sup>4</sup> *Ibid.*, 1911, p. 160.

<sup>5</sup> *Ibid.*, 1905, p. 157. For these burials see also *Monumenti Antichi*, xv, 273-314.

<sup>6</sup> Cf. *Opuscula Archeologica*, II, 155.

<sup>7</sup> See *Cambridge Ancient History*, VII, 385-6.



place. Even in Imperial Rome this primitive condition of the Forum was not entirely forgotten as we learn from Ovid :

Hoc, ubi nunc fora sunt, udae tenere paludes  
amne redundatis fossa madebat aquis.  
Curtius ille lacus, siccas qui sustinet aras,  
nunc solida est tellus, sed lacus ante fuit. (*Fasti*, VI, 401-4).

Roman tradition and archaeology are therefore in accord in placing the transformation of the primitive villages into the great city in the latter part of the 6th century, and the new discoveries on the Palatine provide the final link in the chain of evidence. The data now published are too few to form the basis for a full discussion of the character of the settlement on the Palatine. Some idea may be gathered from the early sites in the neighbouring Faliscan territory. A typical example lies on Monte S. Angelo, some 16 miles northwest of Rome. The surface of the hill has been roughly levelled and the slopes artificially scarped, gaps in the natural defences being made good with rude ramparts of stones and earth. The original settlement measured only some 180 yards by 60 yards, but a rather later extension belonged to the same cultural phase. This extension was also fortified with rough masonry and within it were found the remains of huts of the same type as those recently excavated on the Palatine.<sup>8</sup> This is only one of a number of sites examined, all of which belong to the early Iron Age. The general character of the settlement on the Palatine is unlikely to have been very different. The only criticism I would put forward concerns a point of detail. The photograph suggests that the channel surrounding the hut may have been the bedding trench for a wall of wattle and daub or some other light material rather than a drain for carrying off rain water. But the determination of this and other points must await a fuller report of the excavations. For the moment we must clothe the bare facts with the poetic vision of one who was steeped in the traditional lore of his city :

Hinc lucum ingentem, quem Romulus acer Asylum  
rettulit, et gelida monstrat sub rupe Lupercal . . .  
Nec non et sacri monstrat nemus Argileti . . .  
Hinc ad Tarpeiam sedem et Capitolia ducit,  
aurea nunc, olim silvestribus horrida dumis . . .  
  ad tecta subibant  
pauperis Evandri, passimque armenta videbant  
Romanoque Foro et lautis mugire Carinis . . .  
  et angusta subter fastigia tecti  
ingentem Aeneas duxit. (Virgil, *Aeneid*, VIII, 343-67).

## A ROUND WOODEN HOUSE IN SOMERSET

A small Roman site at Catsgore, about two miles south of Somerton, was examined by the Somerset Excavation School in September, 1950. Masonry of two periods was found. The earlier buildings belonged to a small farmhouse with half-timber walls on a stone base, and a thatched roof. This was erected about A.D. 100. The later work had been much destroyed by ploughing and its plan was not recovered.

The cleaning of the surface of the subsoil in front of the farmhouse brought to light part of a circular trench, together with two stone packed post holes. This trench passed

<sup>8</sup> *Monumenti Antichi*, IV, 33-94.

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under one of the walls of the farmhouse. Further search disclosed the trench at various points on the circuit and revealed three more post-holes, similar to those first discovered, but without stone packing.

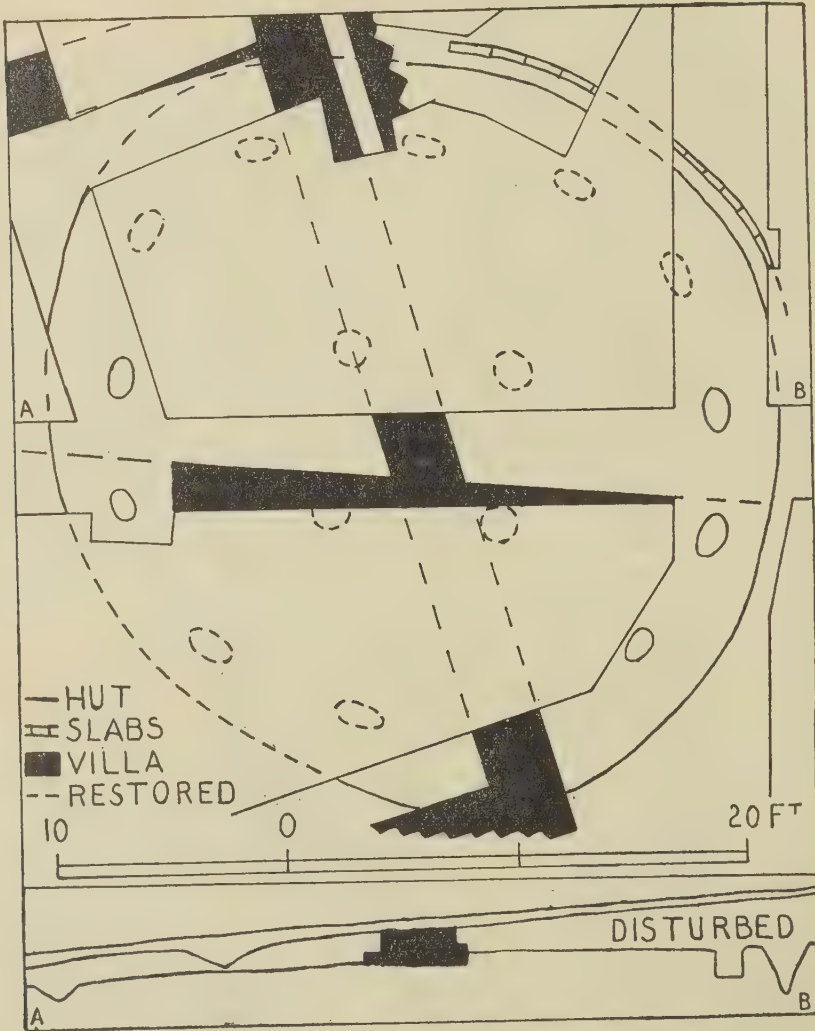


FIG. 1. WOODEN HOUSE AT CATSGORE, SOMERSET

The plan (FIG. 1) showed a roughly circular trench, 32-34 feet across, V-shaped in section and about 1 foot wide. The post-holes lay on a circle some 3 feet within the trench; those found were 6 feet apart and 13 would have been required to complete the ring. The remains are those of a timber house, the trench forming the bedding for a



light wall, probably of wattle and daub. The position of the ring, so close to the wall, implies a central structure, as at Little Woodbury. It was not possible to look for this; three of the four holes of the square probably lay under the later walls and the fourth was covered by the dump.<sup>1</sup> The house lay on a slope, but the bottom of the wall trench was level. Ploughing and the subsequent occupation of the site have destroyed all traces of the floor, but it is clear that the soil on the upper side of the house must have risen above this level; it was retained by slabs of lias set on edge along the outer margin of the trench. The more substantial, stone packed post-holes represent the sides of the passage leading to the entrance, which would have been covered by a gable rising above the general slope of the conical roof. No trace of wood was found in the post-holes or in the wall trench. The timbers had been withdrawn when the farmhouse was erected. Within the area of this house the holes and trench were filled with clay from the subsoil and contained little pottery. Outside the filling included more soil, some rubbish and a fair quantity of pottery. This pottery showed that the house had been occupied between A.D. 70-100. It does not include the earliest Roman types found at Ilchester, which lies only three miles away. (The plan is published by kind permission of the Somerset Archaeological Society, under the auspices of which the Excavation School was organized)

C. A. RALEGH RADFORD.

*Note by the Editor.* The importance of this discovery is due to the fact that the round hut must have preceded the stone-built house that overlies it. If, as stated above, the hut was dismantled when the house was built on its site, it is a fairly safe inference that the change marks an improvement in living conditions due to improved methods of building introduced by the Romans. There was a similar superposition at the Roman house at Iwerne, Dorset, excavated by General Pitt-Rivers (*Arch. Journ.*, vol. CIV, 1948, 48-62). A Belgic hut, probably oval or round and dating from the early part of the 1st century A.D., was found under the Roman villa at Park St., near St. Albans (*Arch. Journ.*, CII, 23-5). In date a closer parallel is provided by the rectangular wooden building found below the Roman villa at Ditchley (*Oxoniensia*, I, 37-9. It is thus probable, as Haverfield long ago suggested, that many 'Roman Villas' were built and inhabited by prosperous native Britons, not Romans.

## WILHELM OSTWALD (1853-1932): A NOTE ON THE HISTORY OF CULTUROLOGY

Professor Wilhelm Ostwald was a distinguished chemist. He was born in Riga, educated at the University of Dorpat. He served as professor at Riga and later as professor of physical chemistry at the University of Leipzig. He was awarded the Nobel prize in chemistry in 1909.

Ostwald was invited to prepare two lectures on the occasion of the inauguration of the Rice Institute of Houston, Texas. These addresses, translated into English by Prof. Thomas L. Blayney of the Rice Institute, were published in *The Rice Institute Pamphlet*, vol. II, no. 3, November, 1915. They were entitled, 'The System of the Sciences' (pp. 101-90) and 'Principles of the Theory of Education' (pp. 191-221). Vol. II, no. 3 of the Pamphlet contains only these two papers by Ostwald.

In 'The System of the Sciences' Ostwald undertakes to explain the sequence in which the sciences have taken form and developed. In this endeavour he follows both

<sup>1</sup> The restoration based on the proportions of Little Woodbury shows this central structure seven feet square (*Proc. Prehistoric Society*, VI, 86).

Comte and Spencer rather closely: the social sciences have come after the biological sciences because the former are more complicated than the latter. The physical sciences, being more simple than the biological, preceded them, etc.

But, in the pyramid of the sciences, at the top of the hierarchy, Ostwald places not sociology, but *culturology*. He points out (as Kroeber did later in 'The Possibility of a Social Psychology') that the term *sociology* is an inadequate and misleading one. It is not *association*, Ostwald argues, that is the distinctive characteristic of man, but *culture*. He points out further that this distinctive character of man can be manifested by a single individual, thus exposing the fallacy of those who argue that 'it takes at least two to make culture'. Ostwald notes that the science of civilization, or culture, has 'usually been designated by the improper term of *sociology*'. But, because of the inadequacy of this designation, he 'proposed, therefore, long ago to call the field in question the science of civilization, or *culturology*' (*Kulturologie*). This is on p. 167; he also uses the term on pp. 168, 169, 192, 193, 205, etc.

Ostwald takes a materialist's view of man and culture. Culture, he says in effect, is a mechanism for harnessing energy and for putting it to work in the service of man. Culture grows when and as additional amounts of energy are harnessed, etc.

I have not yet located the first instance in which Ostwald 'long ago' proposed to call the science of culture '*culturology*'. Any help in this direction would be much appreciated.

It is interesting to discover in the writings of this chemist a point of view and concepts relevant to the scientific study and interpretation of culture that are much in advance of many, if not most, of the professional anthropologists of the present and of recent decades.

LESLIE A. WHITE.

## AN ANCIENT FISH-WEIR AT BALLYNATRAY, Co. WATERFORD, IRELAND

With the passage of time many old and interesting methods of fishing have died out or are in the process of doing so. Frequently there are obvious reasons for the disappearance of a particular type of fishing engine. This is so in the case of some of the so-called sprat weirs in the estuary of the Cork Blackwater. In days gone by many sprat weirs existed in the lower portion of the estuary of that river but only one survives to-day. In the first place most of the weirs were constructed almost entirely of wattling and the local people now have difficulty in procuring sufficient suitable material for this purpose. Recent increases in the cost of labour have probably also rendered it uneconomical to erect and fish such weirs. Ballynatray weir, the sole survivor, is kept in operation because its owner has an interest in fish and fishing. Moreover, he has a plentiful supply of material on his estate for its maintenance.

The term 'sprat weir' is somewhat of a misnomer for the weir is used, as will be shown later, for the capture of certain white fish as well as for sprats. Although the underlying principle is the same these weirs differ considerably from engines used over a century ago for the capture of salmon. It is well known that with a rising tide some fish have the tendency to move on shore or upstream into an estuary and as the tide falls they return again seawards. Primitive tidal fishing weirs consist of a V erected between tide marks in such a way as to trap fish on a falling tide (see FIG. 1). Locally the Ballynatray sprat weir is known as the 'Abbey Weir' from its proximity to the ruins of the abbey of Molanna (FIG. 2). Formerly this abbey was situated on an island but some time ago, probably in the 18th century, the channel on the west side of the river was reclaimed and now forms part of the lands of Ballynatray. The predecessor of this weir



## NOTES AND NEWS

may be one of the fishing weirs mentioned in a number of records from the time of the dissolution of the monasteries until the year 1654, when the so-called Civil Survey was made. In this survey there is a reference to fishing weirs as follows :—‘ Temple Michael

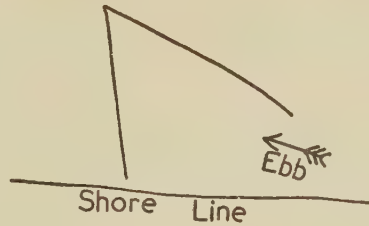


FIG. 1. DIAGRAMMATIC SKETCH OF PRIMITIVE WEIR

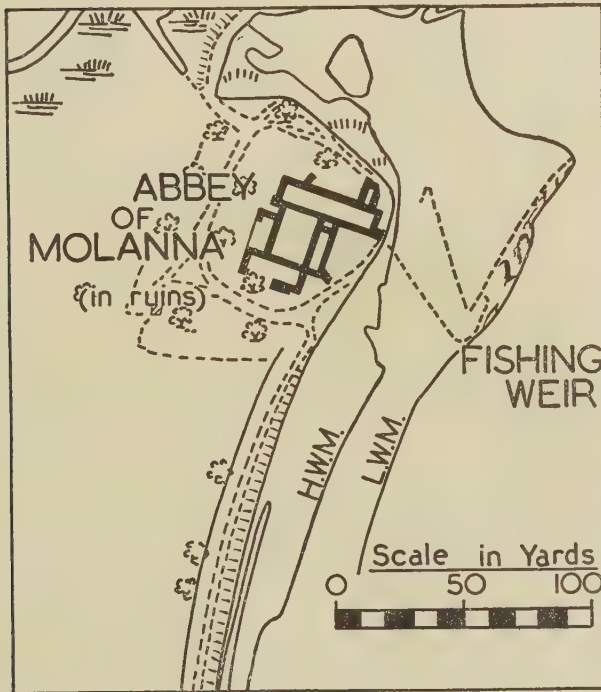


FIG. 2. SKETCH PLAN OF BALLYNATRAY SPRAT WEIR (ABBAY WEIR) AND ITS SURROUNDINGS  
(based on the Ordnance Survey by permission of the Minister for Finance)

and Ballinatray. Old weirs called Curraghegaule in Ballynatray, also in T. there is a small castle and fishing weares.’

The layout of this weir may be illustrated most conveniently in the form of a diagrammatic sketch (FIG. 3). Two long walls of wattling (the mode of construction of which

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will be described later) are placed between the tide marks. One wall, called the *flood wing* is erected roughly parallel to the contours of the shore line (FIG. 2). A second wall, the *shore wing*, is erected at an angle of about 45 degrees to the flood wing, a gap of about 9–12 in. being left between their adjacent ends. The inner end of the shore wing is close to high water mark and has attached to it a short wall almost parallel to the shore but directed downstream. Another wing is erected downstream of the shore wing, the outer end of this wall being carried round in almost a semi-circle to meet a structure known as the *gate* to form the *pound*. The shore end of this latter wall is carried just above high water mark. A grating is provided in the wall of the pound, the function of which will be described later. Approximate measurements of the flood and shore wings are 75 yards and 48 yards respectively. Adjacent to the gap the shore wing is about 7 feet high but gradually diminishes in height towards high water mark being about 4 feet at its inner end. The height of the flood wing is about 7 feet throughout its length.

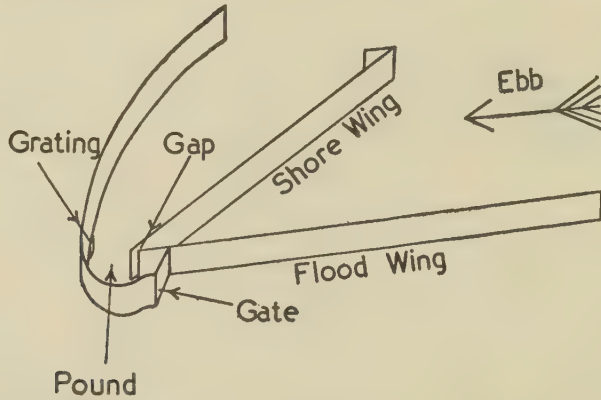


FIG. 3. DIAGRAMMATIC SKETCH OF THE BALLYNATRAY SPRAT WEIR (ABBAY WEIR) SHOWING SALIENT FEATURES (not to scale). THE ARROW INDICATES DIRECTION OF EBB TIDE

Each wall was constructed by driving stakes vertically into the muddy sand at intervals of about 2 feet and entwining small sticks between the stakes in such a way as to form a close basket work (see PLATE II). In order to make the weir 'fish tight' in its lower part some small meshed wire netting is frequently attached to the stakes. The gate consists of an oblong frame to which wire netting of small mesh is suitably attached. A small grating made of small meshed wire netting on a wooden frame provides a method of clearing the pound of weeds, sticks and other debris and is also a free passage for fish at certain times. To strengthen the walls, which naturally offer great wind and water resistance, and to hold them in place, large stones are piled at the base of the stakes. In addition other stakes called *gellogs* placed at angles of about 45 degrees to the horizontal are attached to the vertical stakes at one end, the other end being fixed into the ground by means of large stones.

This weir operates automatically with the ebb tide. Fish move upstream with the flood tide and as soon as the tide ebbs they tend to return downstream unless the discharge from the freshwater portion of the river is sufficient to entice certain species to continue upstream. In doing so they come between the flood and shore wings and they



then proceed through the gap into the pound from which theoretically they could escape, but in fact they seldom do so. When the tide recedes sufficiently the fish can be removed by hand from the pound. As mentioned earlier the grating in the pound can be removed to eliminate debris, etc., from the weir or to provide a free passage for fish. The gate can also be readily removed to permit the free passage of fish. During the run of salmon smolts to the sea in the months of April to June the weir is not normally fished, a free passage being kept open.

A number of other sprat weirs were formerly operated in this region. Most of them were ebb weirs, i.e. they only took fish on the ebb tide but a small proportion of the weirs were, however, flood weirs fishing only on the flood. As fish seldom left the pound it mattered little that the fishermen had to wait until low tide to empty the weir of its catch. All the Blackwater weirs established in the lower part of the estuary were not of this type; some were much more simple and resembled the weirs now fished in Waterford Harbour and the River Suir.

What kinds of fish are taken in the Ballynatray sprat weir may be asked? Certainly in days gone by sprats were the most important fish in the catches but in recent years other fish have apparently surpassed them in importance. Whiting, codling, pollock, coalfish, haddock, flounder, dabs, plaice, bass, mullet, shad, herrings, mackerel and a few other fish are taken from time to time.

This weir is certainly one of the oldest fishing engines existing in Ireland to-day and even if the erection were permitted by law the great expense of preparing a suitable site and providing the necessary material would be likely to preclude anyone from attempting to erect similar structures to-day.

ARTHUR E. J. WENT.

## BASQUE 'LIAS' OR HAND-PLOUGHS (PLATE III)

The illustration shows two examples and was kindly supplied some time ago by one of our readers. They were obtained from Biriadou, a small frontier village on the Bidassoa, and are now on loan in the museum of St. Jean-de-Luz. A pair was presented to the museum of the Society of Antiquaries at Newcastle-upon-Tyne in 1908; and we print below (by permission of the Society) the description of them by the donor, Dr T. M. Allison, in *Proc. Soc. Ant., Newcastle-upon-Tyne*, 3 Ser. III, 1909, 158-9.

'The "lia" or "lai" (lyah) is a square-topped iron fork. It has two chisel-like prongs, 1 foot 10 inches long, 1½ inches broad, and 4 inches apart. These are connected by a horizontal iron bar. The handle, however, is not in the centre of this bar but at the corner, the outer prong being continued to form a 6 inch iron socket. Into this is fitted a wooden handle projecting about the same distance. The tools thus resemble elongated parallelograms, open at the bottom, with a handle at the outer corner, i.e. they are right-handed and left-handed implements. The prongs are slightly, and the handles distinctly, curved forward, so that the front is somewhat concave. The weight is considerable, the two implements scaling together some nineteen pounds.

'They are used in the following way. A "lia" is taken in either hand, the handles being outermost, and the tools are raised high in the air. Then they are "jabbed" forcibly into the ground and worked into the soil. Next they are simultaneously levered *backward*. Then with the right foot the right tool is pushed (like a spade) under the sod, whilst the other *lia* is placed in the furrow by hand. Then lastly a combined levering movement takes place *forward*, and the soil is turned over, the sod being often pressed down by the foot. This method of ploughing effects its purpose from behind forwards, whilst the plough of course turns over its work laterally or sideways.

'I should say that four men with *lias* would plough almost as much land as a man and a woman working with two oxen, the second method of tillage in the Basque provinces. The latter method is of course much the less laborious . . . Sometimes a row of four may be seen preparing the land for maize, for which the *lia* is specially employed, and it is most interesting to watch these handsome Basques . . . wielding the eight implements in perfect time, and turning over a long continuous sod, hour after hour, with almost machine-like precision. Speaking generally the Basques may be said to inhabit the beautiful hill country stretching from Bayonne in France to Bilbao in Spain; and the *lia* seems almost exclusively confined to this region, i.e., it appears to be a purely Basque agricultural implement.'

## Important New Books and Articles

*The inclusion of a book in this list does not preclude its subsequent review*

- DATING THE PAST: an Introduction to Geochronology, by FREDERICK ZEUNER, 2nd edn., revised and enlarged. Methuen, 1950, 30s. [Not only an indispensable book of reference for all archaeologists, but also a fascinating one to read. This edition has been brought up to date. Reviewed in ANTIQUITY No. 79, Sept. 1946, 165-6].
- REALLEXIKON FÜR ANTIKE UND CHRISTENTUM: ein Sachwörterbuch zur Auseinandersetzung des Christentums mit der antiken Welt. Herausgegeben von THEODOR KLAUSER. Hiersemann-Verlags G. m. B. H. Stuttgart, 1950. [The choice of contributors is a guarantee of the high standard of the contributions, which are illustrated. The range of the R.A.C. will be from the relevant Hellenistic features down to the 10th century].
- GLI ABITATORI PRIMITIVI DEL PALATINO attraverso le Testimonianze archeologiche e le nuovi Indagini stratigrafiche sul Germalò: da S. M. PUGLISI. *Monumenti Antichi* XLI, 1-98. [An account of the new excavations and a re-examination of the earlier material associated with hut-foundations dating from the 8th to 6th cent. B.C. See the article on same in ANTIQUITY No. 95, Sept. 1950, 119-21].
- HANDBUCH DER ARCHEOLOGIE, 4 Lieferung: die Denkmäler. (Jüngere Steinzeit und Bronzezeit in Europa und einigen angrenzenden Gebieten bis um 1000 v. Chr.). Beck, München, 1950: 402 pp., 95 maps and figs., 56 half-tone plates. [This part of the famous Handbuch covers (1) Europe and adjacent regions (excluding the Aegean and Italy) by Oswald Menghin, pp. 5-176: (2) The Aegean, by Friedrich Matz, pp. 179-308: (3) Italy with Sardinia, Sicily and Malta, by Guido von Kaschnitz-Weinberg, pp. 311-402. A full review is being prepared].
- OLD DANSKE TEKSTILER (Ancient Danish textiles), by MARGARETE HALD; Gyldendal, Copenhagen, 1950, 491 pp., 450 figs. and maps, with a comprehensive English summary of 80 pp. [Apart from being an excellent survey from the technical and archaeological point of view, the book covers the whole field of prehistoric weaving, plaiting, knitting, etc.].
- HERZSPRUNG SHIELDS AND GREEK TRADE, by HUGH HENCKEN. *Amer. Journ. of Arch.*, LIV (Oct. 1950) 295-309. [Round bronze shields with nick in concentric ridges of Assyrian type occur in Crete and Samos about 800 B.C. The V-nick recurs in Spain and Ireland; the U-nick in Bohemia, Germany and Sweden: both types in Denmark. Hencken thinks both spread west and north before the Etruscan and Greek colonization of Italy, an important date-mark].



PLATE II



THE WEIR FROM THE SOUTH; BALLYNATRAY HOUSE IN BACKGROUND



THE GAP (ON LEFT) AND GATE (ON RIGHT)

PLATE III



BIDASSOA 'PLOUGHS'

## Reviews

THE PREHISTORIC CHAMBER TOMBS OF ENGLAND AND WALES. By GLYN E. DANIEL. *Cambridge University Press*, 1950. 31s 6d.

This book is 'a general survey' of the tombs, excluding however the earthen long barrows, 'particularly with regard to the problems of tomb form and origins'. It has thus two aspects. The general survey is in the best traditions of accurate Cambridge scholarship, assembling what is known about the tombs and reviewing what has been said about them over the past century; it ends with an inventory of over seventy pages which will be invaluable to future students. It extends also to the distribution of the tombs, to the ritual implied by them, and to the grave-goods they contain.

The last three subjects are indeed not pursued very far. Distribution is rather generally discussed in terms of climate, altitude, etc.; not in detail in terms of the sheltered beaches and habitable lands which the builders of the tombs must so earnestly have sought among the forests and marsh that confronted them. The discussion of funerary ritual is substantially confined to a careful and balanced review of the old controversy as to whether the tombs were used for successive burials. The grave-goods are fully listed, but not illustrated or indexed, and only discussed in a couple of pages. We are asked to await a projected work by Prof. Piggott on this subject, and we happily have Mrs Clifford's scholarly review of the Cotswold material in the Chadwick Memorial Volume. But this gap in the book is a serious one when Dr Daniel comes to draw conclusions on the settlers of whose culture the tombs form a minor part.

In its second, and perhaps to Dr Daniel its more important, aspect, the book is an essay on the thesis that, by typological study of the ground-plans of the chambers of the tombs, we can track the early settlers of Western Britain to their homelands overseas. This is a thesis by which we have all been attracted; and many suggestions have been derived, and usefully followed up, from the patent likenesses in ground-plan between some of the chambers. The game is like that of spotting family likenesses from the family album; in which it must be admitted that the pleasure we get from our successes distracts us from our much more numerous failures. It is when we attempt with Dr Daniel a complete morphological analysis of all examples that we meet the reluctance of man, and of his tombs, to be forced into a mould.

Dr Daniel's examination of the thesis is a model of careful scholarship, and we are unlikely to see it argued equally well again. If he fails, it is because the thesis, in its generality, is untenable and his argument deserves careful review. Of the tombs he studies, some fifty have been excavated, but of only a handful of these have we complete plans deriving from modern excavation. It is thus disquieting at the outset to find Dr Daniel in dissent from the excavators, and removing a side-chamber from one of this handful of tombs and adding a side-wall to another; he may be right, but a difference of opinion, which frequently recurs in his discussion of tombs earlier excavated, points to a dangerous ambiguity in the ground-plans which are to be the subject of his analysis. The danger is naturally increased when we come to the two hundred tombs which have never been excavated at all.

Such is the material. Dr Daniel's analysis of it proceeds by way of an eightfold morphological categorization, but the eight forms fall within one or other of the classes



of the familiar dichotomy : passage-graves/gallery-graves. The practice of dichotomy is an age-long scholastic weakness, and the works of man are seldom so classifiable without violence ; but this is an illegitimate dichotomy, for the two classes, by definition, are not mutually exclusive. This fault in logic arises from the inclusion under passage-graves of the categories of entrance graves and 'undifferentiated' passage graves ; categories of which the members, as is admitted, may in practice be indistinguishable from gallery graves. The fault is not to be remedied by the supplementary criterion that in passage graves burials are concentrated at the inner end : in logic, because the added criterion is not morphological ; in practice, because in hardly any tombs do we know what the distribution of burials was. Nor can the fault be palliated by pleading that the cases ambiguously categorized are few ; for Dr Daniel, in disagreement with Dr Hencken, classifies all the fifty Scilly tombs as entrance graves or 'undifferentiated' passage graves.

The argument will not do ; to the question 'When is a gallery-grave not a gallery-grave' ? it gives the answer 'When it is an "undifferentiated" passage-grave'. Some other criterion must be hidden behind these categorizations, for example behind that of Scilly tombs as 'undifferentiated' passage-graves. It is to be found in the statement that, generally, 'round barrows are associated with the passage grave series' : the Scilly barrows are nearly all round ; therefore, it would seem, they should somehow be brought within the passage-grave series. Thus we now find ourselves involved in a cross-classification, unrelated to chamber ground-plan, and back with Aubrey in the old dichotomy : round-barrow/long-barrow ; which is, it may be noted, one more dichotomy which the tombs we actually excavate obstinately evade. There may be still another criterion, and again a non-morphological one, concealed behind the categorization of the Scilly tombs : from their grave-goods, the tombs are late, and correspondingly appropriate to the passage-grave series, in accord with the fixed dogma of the typologists that passage-graves are later than gallery-graves. That this is a pre-supposition with Dr Daniel is shown on p. 164 by his unconsciously faulty syllogism : the passage-grave of Rudh' an Dunain is pre-beaker ; the earlier Boyne tombs are pre-Rudh' an Dunain ; therefore the Boyne culture was established in beaker-times or later.

Plainly the typology cannot contain the facts, which are appealing from it to unavowed, and non-morphological, criteria ; however trimmed and groomed, they are breaking out of the morphological pattern. The fault is not with the writer, who indeed has done all that it is humanly possible to do ; it is with the thesis. Ground-plans are an element, though not a very important element, or with any certainty an index-element, in the cultures of the early settlers of Western Britain. These cultures are available to us for study as a whole ; and we shall find great diversity. The Scillies, islands late settled and on the route to nowhere, used chamber-tomb burial, and a singularly uniform chamber ground-plan, for five centuries ; and for several centuries after other funerary cults were current on the mainland. A dozen farming families which settled Rousay, a small island in the Orkneys, built at much the same time, and used for perhaps two centuries, a dozen chamber-tombs of marked individuality and still more marked variety. In that tiny community sentiment differed from that in the Scillies ; individuality rather than identity was valued in the planning of the family tomb. Archaeology can do much, and it can recognize two contemporary communities in so small an island as Rousay ; but it cannot hope to trace to their individual sources the traditions of each of the twelve families of the community which built the chamber-tombs.

The study of the English tombs is now behind that of the North Irish and Scottish ones, and the field of work opened up by the survey in this book will give it a new impetus.

But the method that will succeed will be the study as a whole of the culture of the settlements to which the tombs belonged; the very merit of Dr Daniel's exposition of his thesis has shown that the typological study of the ground-plans of the chambers of their tombs will carry us but a little way towards understanding who the settlers were and whence they came. L.S.

# GREAT TOMBS OF THE FIRST DYNASTY, I, EXCAVATIONS AT SAQQARA.

By WALTER B. EMERY, *Service des Antiquités de l'Égypte, Government Press, Cairo, 1949; XI+157 pp., 55 plates of plans and photographs. Price not stated.*

This volume appears as a welcome and exciting addition to the great series on the excavations of the early tombs at Saqqara which Emery began with *The Tomb of Hemaka* in 1938 and followed with *Hor-Aha* in 1939. To one who can remember Firth's first season of work in the Northern Cemetery at Saqqara in 1930 and the task of collecting what little was known as yet of this important area for Reisner's *Development of the Egyptian Tomb* after Firth's untimely death in 1931, it is fully evident in this volume what miracles can be accomplished by an expert archaeologist such as Emery. When Firth's death was followed by that of Quibell who had earlier worked in the archaic cemetery, it seemed that Emery was undertaking a thankless task as he began completing the clearance of a section of the cemetery from which little more could be expected. It almost immediately became evident that a more thorough examination of the ground and particularly the interior of the mud-brick superstructures of the great First Dynasty tombs was to provide us with a wealth of objects which would enormously increase our knowledge of material which could only partly be salvaged by Petrie at Abydos after the inexpert handling in the days of Amelineau.

In spite of this wealth of new material, it may seem to the reader that in many ways Emery's excavations have provided us with more perplexing questions than they have as yet solved. It is all the more to be regretted that his work at Saqqara has had to cease since the war, and one hopes that he may be able to return to the still enormous task of clearing the rest of the archaic cemetery. Dr Reisner had pointed out for a long time that the thorough excavation of this cemetery was probably the last opportunity we should have of recovering decisive material concerning the history and civilization of the first three dynasties. Since then, the remarkable results of Zaki Saad's excavations at Helwan have shown that at least one other site is also capable of producing such material in large quantity. This does not in any way detract from the importance of Saqqara as one of the greatest sites for excavation in Egypt, particularly as concerns its early history. The excavation of the archaic cemetery is work of the utmost delicacy which requires the expert knowledge and technique of which Emery has shown himself to be a master. If at some future date he may be able to continue there, we can then hope for the answers to some of the problems he himself has raised.

We are a little better equipped for comparative material than was the case when Emery began his work in 1935. Not only have preliminary reports of Zaki Saad's work at Helwan appeared (*Cahier No. 3*, as supplement to *Annales du Service*) but in *Kemi*, VII (1938), pp. 11 ff. and VIII (1946), pp. 157 ff. Montet has published the results of the excavation of a small First Dynasty cemetery at Abu Roash which had remained virtually unknown since the work was done in 1913-14. The present writer reviewed very briefly some of the problems raised by these discoveries in the *Archaeological News* section of the *American Journal of Archaeology*, LI (1947), pp. 420-1. There the belief was expressed that the evidence still pointed towards the recognition of Abydos as the Royal Cemetery of the greater part of the kings of Dynasty I and part of those of Dynasty

II. Emery in his new volume has refrained from expressing any theories on this matter, until more information can be brought to bear upon it. In his earlier volume he seemed inclined to accept Saqqara as a royal burial ground of Dynasty I containing the tomb of Hor-aha who as Menes was recognized as the first king of Dynasty I.

The current volume presents a curious example of the difficulty of identifying the ownership of a tomb from the name of the man inscribed upon mud sealings found in it. For in tomb 3036 the name of Ankh-ka occurs with a characteristic series of titles on the sealings. It is not surprising that this same man's name should appear upon a sealing in the tomb of Wedymuw at Abydos, since this is the king whom he served as an important official. It is puzzling, though, that at Abu Roash the same sealings of this man should be found in Tombs III, IV and XV, as well as another which probably belonged to him in Tomb VI. Ankh-ka was administrator of a district, the name of which is surrounded by a crenellated wall that in early times seems to indicate a great vineyard rather than a town. It may be as controller of these vine lands that his seal was placed upon their products in the stores of the royal household and the general administration, the products being then distributed as gifts by the king to supply such tombs as those at Abu Roash. One of Ankh-ka's seals was actually found in the tomb of Hemaka (Emery, *Hemaka*, p. 64, Fig. 25) who was also administrator of the same district. Since Hemaka's name occurs upon other objects besides sealings in his tomb there is no reason to doubt its identification. The adjoining tomb 3036 is of similarly imposing size and must also have belonged to a great man. Since Ankh-ka seems to have been associated with Hema-ka, perhaps as his predecessor in office, Tomb 3036 probably really belonged to him as his sealings found in it imply.

Perhaps the most important material in this volume is the large collection of copper tools and implements from Tomb 3471 of the reign of King Zer. These produced an unforgettable impression when they were first laid out on the workroom tables at Saqqara after cleaning. They cover a wide range of practical forms from large adzes, chisels and knives to tiny needles and awls and in most cases still have their wooden handles preserved. There are also some interesting vessels of which a spouted container is perhaps the most unusual in shape. From this tomb came also the fragments of a wooden bed and parts of the poles of a wooden canopy that must have resembled the one found in the tomb of the Fourth Dynasty Queen Hetep-heres I at Giza. This form is now known to have existed throughout the Old Kingdom since a badly preserved example of Dynasty III was found in the store chamber of the tomb under the southern enclosures wall of Zoser's Step Pyramid, while pictures of a canopy with a bed set up beneath it are known in the later tomb chapel scenes at Giza.

One of the smaller Dynasty I tombs, No. 3121, has preserved valuable fragments of early painting. Brightly coloured patterns imitating matting decorated the walls of the chapel. The designs are reproduced excellently in colour on Pl. 50 and anticipate the geometric patterns that were used to cover the 'Palace-façade Panelling' of later Old Kingdom tombs and the plastered brick niches in the Third Dynasty tomb of Hesy-ra. No green is used but it is interesting to note that blue is employed along with red, yellow and black. The colour is a greyish blue in the plate but it would be interesting to know whether this is the usual bright Egyptian blue pigment hitherto not known earlier than the reign of Cheops.

An interesting object of far-reaching implications is the buff-ware handled jug with painted decoration illustrated in Fig. 68 from Tomb 3120, dated like 3121 from sealings of Qay-aa. Emery uses the term 'Aegean' to describe this vessel following Petrie's name for similar sherds decorated with geometric patterns first discovered by



him at Abydos. Identical decorated ware is now known from the Syrian site of Tell Judeideh in the Antioch Plain and seems to have been imported into Egypt from the north-east, probably from Syria, like the slender handled-jugs and the Old Kingdom handled-jars with combed surface decoration. It is perhaps worth mentioning in this connection that Mr Lucas analyzed the contents of one of the Fourth Dynasty handled-jars with combed surface from Giza (G 2140 A; Reisner, *Giza Necropolis I*, p. 437) and found that it consisted of resin derived probably either from the Cilician fir or Aleppo pine, coniferous trees which are found both in Syria and Asia Minor. The painted ware has not been found in Egypt after the First Dynasty. Emery's example is unusual in having a bird inserted between the more usual dot-filled triangles on the shoulder of the vessels.

The large open bowl of schist from the tomb of Sabu, No. 3111, is a remarkable object, although tantalizing fragments from Abydos had already shown that the vase-maker sometimes fashioned the stone into elaborate forms as though he were modelling in clay. Part of a slate dish found at Abu Roash (Montet, *Kemi VIII*, p. 176, Pl. v) shows a similar complex form. In the Saqqara example, three sections of the side of the bowl are turned in towards a hollow cylinder in the centre, although the narrow rim is left intact around the edge of the dish. Another important contribution of the tomb of Sabu lay in the skeletal evidence. For the first time part of the skull of one of these important men of the early period had survived.

Finally there is the architectural evidence so attractively presented in Emery's beautiful drawings. This volume provides an interesting sequence of tomb types with more evidence to illustrate the beginnings of stone working already testified by the rock-cut chambers of Hemaka and the stone-lined burial chambers at Helwan. The two tombs 3120 and 3121 present a more developed type than has hitherto been known from Dynasty I with their two-niched brick mastabas, corridor chapels and their burial chambers hollowed out of the rock and approached by a stairway. It is difficult to comment adequately upon the extraordinary stepped structure of brick incorporated into the super-structure of Tomb 3038 which was later faced like the other panelled mastabas of Dynasty I. Emery has called attention to the stepped structure represented on inscribed objects from Abydos of King Az-ib to whose reign Tomb 3038 is to be dated. The grain bins of this tomb are repeated again at Helwan and Abu Roash.

The tombs described in this volume have produced no sculpture. There are no reliefs to set beside the extraordinary disk from the tomb of Hemaka with its marvellously carved hunting scene in various coloured stones. There is only a schist palette incised rather roughly with a figure of the king (without royal attributes) striking down a foreigner and the forequarters of a lion. This comes from the same tomb which produced the copper implements (No. 3471). One looks forward to the publication of the secondary tombs at North Saqqara, many of which are of Dynasty II. Their chapels contained a number of the so-called 'primitive niche-stones' in which originated the offering table scene of the tablet of the later false-door. These early inscribed stones have now also been found at Helwan, but two examples at Saqqara are of outstanding workmanship, one of them still retaining its original colour.

WM. STEVENSON SMITH.

FISHING IN MANY WATERS. By JAMES HORNELL. *Cambridge University Press*, 29 September 1950. 204 pp., 36 plates and 44 text figures. Price, 30s.

The death of James Hornell is a real loss to the study of marine archaeology. Like the late H. H. Brindley, he was a great collector of information on every kind of marine subject. He was also a brilliant photographer, as the plates in this book will show.

## ANTIQUITY

The book itself was published after his death. It is a remarkable work, which should be consulted by all readers who have to deal with past or present fishing or hunting communities of Western Europe. In it the student will learn how to distinguish a harpoon from a fish spear; a confusion which is frequently seen in text books. He can also learn how people fish from dug-out canoes. The use of simple nets, basket traps and weirs, all still to be found in Britain, is described. They are not only to be found, but can still be seen in almost identical forms to those of Fiji or Africa.

Most of Hornell's work was done on the coasts of the Indian and Pacific Oceans. The reader will find all manner of unexpected information, such as the manner of catching and curing 'Bombay duck,' which he may or may not know to be a fish; or how the Japanese deal with bonito.

The chapters are really short essays on various aspects of fishing which have taken Hornell's fancy. Some of them are written in a strictly scientific manner with no frills about them. Others contain delightful scraps of anecdote, which I for one would have liked to have seen expanded. The disgust of the Tongan deck passengers of a four-masted schooner when the carcase of a shark was thrown overboard before they could eat it, makes an excellent introduction to the shark-fishing chapter. The rival claimants for the tail too; the mate who wanted to nail it to the bowsprit-end to ensure good luck and fair winds, and the cook who esteemed it a delicacy; all serve to bring out points of importance, either in superstition or gastronomy.

The book is full of interest from first to last. Whether Hornell is dealing with cuttle-fish or edible sea-urchins, crabs or sword-fish, there is something to be learnt. How many yachtsmen, for instance, who so often admire the fine French Tunny-fishing Ketches, know that the fishery is quite recent?

This is not a text book. The reader may turn to any chapter and read the book in any order. If he likes to learn how cormorants are used as retrievers he may do so, or if he prefers catching crocodiles he can turn to that.

The archaeologist or anthropologist who studies this book with care will find many things to make him scratch his head. Why are identical ways of catching fish, often of a highly complicated nature, found in India and Central Africa? The old question of diffusion, or invention in different places, crops up again and again. Hornell does not advance theories. He is the teller of the mystery story. The audience must supply the explanation.

T. C. LETHBRIDGE.

**THE NORSE DISCOVERIES AND EXPLORATIONS IN AMERICA.** By EDWARD REMAN. Edited by ARTHUR G. BRODEUR. Published by University of California Press for whom Cambridge University Press act as agents. (*Published 10 February 1950, pp. 196, 1 map*). Price, 26s.

The author of this book completed the manuscript shortly before his death in 1945 and sent it to the editor, who recast and rewrote the entire work in order, as he says, 'to purge the text of those qualities of idiom and phrase which few men not born to English speech can shake off'. It is a pity perhaps that he could not have shaken off 'motivate' at the same time. This is a petty grouse no doubt, but one must begin a review somewhere.

The book appears to me to be much the best study of the evidence for the courses of the early Norse voyages to America that I have yet read. All the relevant saga accounts have been translated once again with some slight variations in the meaning of words and phrases.

The author stresses a most important point in attempting to estimate the relative

value of the saga accounts. He maintains, and in this he is clearly right, that the traditions handed down in Greenland and which have been preserved in the *Flateyrbók* (*Groenlendinga þáttur*) are naturally more reliable in their accounts of the voyages of the Greenlanders, the children of Eric the Red, than those preserved in the Icelandic version, which is known as *Eirík's Saga*. On the other hand, he states with equal conviction, that those events which concern the voyage of the Iclander, Thorfinn Karlsefni, will have been recorded with greater accuracy in this *Eirík's Saga*, which was made for Icelanders and preserved because it concerned the doings of Icelanders. Using this method of estimating the value of his sources, he deduces that Eric's children, Leif and Thorvald Eiríksson, explored an entirely different part of America to that which Karlsefni attempted to colonize. He brings Leif and Thorvald down the west coast of Newfoundland and into the Bay of Fundy; while he takes Karlsefni right across Hudson's Bay and then down to the bottom of it. He rightly stresses the importance of the climatic changes, which have taken place since the 11th century, in forming any estimate based on the distribution of vegetation. On the other hand, he doubts, I think wrongly, the possibility of ever finding any trace of the Norsemen's winter houses. Karlsefni's settlers at any rate are certain to have constructed long winter houses, with turf walls which should still be visible. There is, in my opinion, a better chance of finding Karlsefni's winter houses at Straumfjord and Hóp, than there is of ever reconstructing his voyage from the Saga material. From the Saga account, I should rather doubt that the expedition ever crossed Hudson's Bay to the Chesterfield Inlet and Nelson River. It may have explored Ungava Bay, or it may have passed south to Hamilton Inlet. The Saga account does not seem clear enough for anyone to be certain. The author may be perfectly correct, but he has not settled the question.

On the visits of Leif and Thorvald to the coasts of Maine, I feel he is on surer ground. His explanation is most plausible. There is, however, less chance of ever finding Leif's houses, than of finding those of Karlsefni's expedition. There would be no harm, however, in somebody undertaking a search or even making a careful study of air-photographs of the shore opposite Grand Manan Island. The quest is by no means hopeless. Some trace of a long house, which was occupied for at least two winters, might well remain if only as a crop mark. I do not know the country, however, and have no means of estimating the possibilities.

Anyone interested in this fascinating set of problems should read this book. It is the best attempt at finding a solution that has yet been made. T. C. LETHBRIDGE.

ANGLO-SAXON GOLD COINAGE IN THE LIGHT OF THE CRONDALL HOARD. By C. H. V. SUTHERLAND, M.A., D.LITT. pp. 106, pl. v. Price 25s net. Published on behalf of the Visitors of the Ashmolean Museum, Oxford, by *Geoffrey Cumberlege, Oxford University Press, London, 1948.*

The appearance of this book is really only the last chapter in what adds up to an extremely interesting and even exciting story. The first chapter was the discovery of the hoard at Crondall, Herts, in 1828. The second was the passing of the hoard from the family of the finder, C. E. Lefroy, into the collection of the late Lord Grantley in 1895. The third was the appearance of the hoard in public auction after Lord Grantley's death, the fourth the purchase of the hoard in its entirety by the Ashmolean Museum. Thanks to the energy of the Museum staff and the public-spirited cooperation of Messrs A. H. Baldwin & Sons, and others, a hoard, of unique importance, has found its home in one of our great public Museums as a memorial of that great antiquary, Sir Arthur Evans, whose wide ranging interests did not neglect the coinage of our own Dark Age in Britain.



Of the original hoard 97 coins out of 101 survive. The remaining four—an ancient forgery and three blanks, are no longer to be found, nor are the chains and clasps that were discovered with them. To the elucidation of these 97 coins Dr Sutherland has brought all the resources of the modern expert. He has catalogued them with precision, noting weights, die-positions, die-links, and so on, and has woven into his study all such other early gold of the Anglo-Saxons as he could discover. What he offers us, then, is really a Corpus of the little pieces that are usually called Thrymsas (with a few larger pieces, *solidi*, appended). In one sense his work will be final for our generation—it will not need to be done again. But it will not be final in the sense of putting a stop to discussion. It is far too good a book not to provoke further research on this subject.

Twenty-one of the Crondall coins are Merovingian from Gaul: the remaining seventy-three are either certainly or presumably Anglo-Saxon. If the latter, some definitely copy Merovingian prototypes, others work equally definitely in the Roman tradition. Occasionally, a Roman bronze coin of the 4th century is rendered in a more or less blundered version on the gold. A third class consists of coins struck at London or their derivatives, a fourth of the Witmen coins, with diademed head and trident on obverse and cross on reverse. Then come very rare issues with the names of kings, of which Pada of Mercia is the most interesting—though he was not represented in Crondall. A few miscellaneous issues close the account. One small class, possibly to be assigned to York, was not in Crondall.

The five admirable plates provide the quickest way to a knowledge of the coinage. When the eye has grown accustomed to their style and learned to distinguish its shades, the descriptions can be called in to make knowledge fuller and more sure. Some features impress themselves at once—the narrow range of themes, the limited competence of the artists: their will to find expression forcing its way through difficulties, and the wide difference to be noted among them. One is left with a very vivid impression of the qualities of art in Britain at the turn of the 6th and 7th centuries, which should be invaluable to students of archaeology and art in general.

Dr Sutherland rightly lays stress on the strict limitation of this coinage in places. It was certainly struck in London and in Kent. The mint at York is uncertain, the mint at Winchester improbable. Extension of the coinage into Mercia (King Peada) comes comparatively late. The dates will be from about A.D. 575 to 675 (p. 67). The coinage will linger as a result of increasing contact with Gaul towards the end of the 6th century. It will end when it gives place to the silver coinage of sceatas. The absence of any Anglo-Saxon gold from the Sutton Hoo burial, *c.* A.D. 650 or later, is not surprising; for that burial is well outside the common area of circulation. Against Dr Sutherland's attributions both to time and place there seems to be little to be said. He has given us sound foundations.

In the course of his work and in a final 'general summary and reconstruction', Dr Sutherland takes account of some of the main problems of the Dark Age and relates these to his own particular subject. He says, and says very well, the little that can as yet be said with certainty. After the Roman legions had left, there is a period of coinage of 'survival', the last Roman issues continuing to circulate. Then comes a real Dark Age, marked, we believe, by many imitations of Roman bronze, but with no ordered coinage of its own. This darkness at last begins to break under Merovingian influence with the coming of the thrymsas. After about a century, they give way to the silver sceatas, which were themselves to yield within a century to the silver penny. We shall hope, in the sequel, to learn more of this barbarous coinage that preceded the thrymsas. No one who studies attentively the copying of Roman types and the close similarity of

some of the heads to those found on barbarous bronze can have any serious doubt that such barbarous bronze was in common issue in the 6th century. But we have still almost everything to learn about it. We hope, too, in time, to learn more of the 'survival' history of the late Roman silver coin, 'siliqua'. It has struck me of recent years that the Merovingian silver, the *Saigas*, are not nearly as well known to English students as they ought to be, and that they may have an important bearing on some of our problems.

Personally I have had much instruction and much delight from Dr Sutherland's book. It is admirably produced, the technique is well-nigh perfect, and the argument advances clearly and certainly from point to point; its long-term success, based on its solid merit, seems to be assured; but it is to be hoped that readers will not deny it a shorter term success also, if not as a 'best-seller', at least as a book in steady demand. Such works as this cost time and money, and support from the reader classes strengthens the writer's hands.

The feeling with which the book leaves me is that Britain of A.D. 600 was beginning to develop a new civilization, derivative from Rome and Gaul and yet with some impulse of its own. That that civilization was not without form and value and that it may be studied with some hope of success. Archaeologists, so far, reporting honestly, if perhaps a little too pessimistically, on the negative side—on what they do not find, have induced a belief that the Dark Age in Britain was very dark indeed. Is it not fairer to say that it can now be seen like a tunnel, with light at both ends and not too long to receive a little light into most of its recesses? And does not Dr Sutherland offer us, from his particular corner of archaeology, something modest in extent, but quite clearly defined and quite refreshingly positive?

HAROLD MATTINGLEY.

OPUSCULA ARCHAEOLOGICA VI: EDIDIT INSTITUTUM ARCHAEOLOGICUM ROMANUM REGNI SUECIAE. (Swedish Archaeological Institute in Rome xv) Lund, 1950. 4to, pp. 272, illustrated. Price 70 Swedish crowns.

T. B. MITFORD. 'New Inscriptions from Roman Cyprus'.

These forty-nine inscriptions were copied in four visits (1936-9), some in the field, some acquired by the Cyprus Museum since the *Catalogue* of 1899. They all belong to the Roman period after 57 B.C., and most of them to the Emperors from Tiberius to the Antonines, when Cyprus was peaceful and prosperous. Most of them are in Greek, and the editor prefixes a note on the principal styles of lettering, which succeed each other with some overlap and confusion.

They deal with many matters—dedications to deities and emperors, monuments to officials and benefactors, completion of buildings and aqueducts, benefactions, mile-stones, tombstones—one of a centurion, another the pathetic epitaph of an infant. A novelty is the welcome-slab from a house-door. There are two mosaics from a sea-side bath-house at Mansura, one of them welcoming the 'sea-lover', the other, a retriever and partridge, giving a new word for 'hunting'.

All are carefully photographed. They offer many small points of language, epigraphy, and history. Swedish archaeologists have done so much for Cyprus, that their help in publishing this contribution is the more acceptable.

MARTIN P. NILSSON. 'Lampen und Kerzen im Kulte der Antike'.

This is an industrious compilation of examples, from Minoan to Christian (pp. 36-III) and from Egypt, Gezer and Carthage to Thrace and Italy. The 'torch-light procession' survives from days before lamps: these are frequently found votive, and a

sanctuary such as Epidaurus had its lamp lighter (IG. iv, 1<sup>2</sup> 742). Candles seem to originate in Italy where tallow competed with oil. The symbolism of light is obvious, and its magical use various. A lamp-display drove off an enemy from Labranda; the first congratulatory 'illumination' was in honour of the praetor, Marius Gratidianus, who had stabilized the exchange! (Cic. de Off. III. 80). But in Christianity arose 'vexed questions of forbidden lights' which are not yet extinct.

A. R. A. VAN AKEN. 'The Cortile in the Roman Imperial Insula Architecture, pp. 112-28.

The houses of Roman Italy are derived from three types; the old Italian house, with more or less covered *atrium* and central tank (*piscina*) suited to a country of moderate rainfall, with roofs draining inward; the Greek court-yard-house with open portico, dependent on a well; and the later apartment-house of several stories, built round a *cortile* which served as a light well. There are also combinations of these, especially where the site or urban limitations compelled.

There has been controversy as to the origin of the *cortile*. Carrington thought it was modified from the native *atrium*, and its balconies (as in a modern Italian or Spanish house) from corridors supported on columns (ANTIQUITY VII, 1933, 133 ff.) Harsh (M.A.A.R. XII, 1935, 12 ff.) thinks the Greek peristyle-house is the only possible source; and Calza thinks the *cortile* has a specific character of its own: in particular it is never turned into a garden. Now, von Aken revives the influence of the peristyle, quoting the large houses at Ostia, where there is less cramping and deformation. He gives numerous illustrations, including Delian and North African houses, and discusses the influence of shops and offices on the plan of the ground floors. The garden-peristyle is in fact built over, as in modern town-houses; while the *atrium* is enriched with columns from the peristyle-house. The garden did not influence the *cortile*, which is purely utilitarian. Later a central feature with recessed shrines, fountains, and marble decoration marks a new development altogether.

WILLY SCHWABACHER. 'Geldumlauf und Münzprägung in Syrien im 6 und 5 Jahrh. v Chr.', pp. 139-49.

Regular coinage does not occur in Syria till the City coins and Persian provincial coins of the 5-4 centuries B.C. But from the 6th century onwards hoards include several phases of anticipation; lumps and ingots of silver, of irregular shape and weight; then, mixed with them, Greek coins, perhaps at first only as bullion, but later (to judge from their types) tested for quality while in circulation. But at El Beida, Sir Leonard Woolley found a mass of Athenian and other coins partly melted down; and this anticipates the local imitation of the most popular Greek issues. Of these far the most frequent are the famous 'owls' of Athens, which travelled as far as India; less common are coins of cities near the Thracian silver-mines. As Greek currency became more acceptable, there were various counter-marks of approval, and eventually local issues with a bearded head of Melkarth replacing Athena. It is a curious and intricate bypath of the history of Greek coinage.

ARNE FURUMARK. 'The Settlement at Ialysus and Aegean History, c. 1550-1400 B.C.', pp. 150-271.

From Ialysus in Rhodes came the first large collection of Aegean Bronze Age pottery, excavated 1868-71, acquired through John Ruskin by the British Museum, about the time of Schliemann's excavation at Mycenae, and published by Furtwängler and Loeschke in 1886 (*Mykenische Vasen*, Berlin). But the digging had been tumultuary, and it was



not till about 1935 that an Italian, Giorgio Monaco, opened a small section of the settlement near the sea at Trianda. This is now carefully interpreted by one of the ablest of the younger explorers of this period, and made the occasion of a general revision of finds and events in the Later Bronze Age of the Aegean and Levant.

At Ialysus are three strata, the lowest wholly Minoan (Late Minoan IB 1550 B.C.) the second and third more and more permeated with mainland intrusions (Mycenaean II A, B, 1500-1410). The Cretan colony was therefore gradually superseded by a Mycenaean.

It is now possible to trace similar changes at Phylakopi in Melos (excavated in 1895-7 and published in 1904). And at Miletus the colonization of the Anatolian mainland was Mycenaean from the first. More complicated is the intercourse of Minoan Crete with Mycenae itself and other mainland sites, as to which there has been much controversy; Furumark deprecates the extreme claims of some for Helladic mainland folk against Cretan enterprise. He regards the 'Shaft graves' as a local development, and inclines also to a mainland origin for the 'Treasures' (tholoi), the earliest of which may be about contemporary with the latest 'Shaftgraves' (Mycenaean IB), and free of any political domination by Crete; though the rise of royal power was closely connected with the process of Minoanization, and then with the rise of a new and definitely mainland culture (Myc. II A-III) with *tholoi* also at Vapheio, Kakovatos, and Thorikos, and rich chamber-tombs around Mycenae.

On these archaeological conclusions, Furumark goes on to sketch in perspective the spread of Aegean influence in the Levant, distinguishing Minoan, Mycenaean, and local fabrics, and discussing the recent conclusions of Miss Helen Kantor (*AJA.* 51, 1947). There is Cretan pottery at Maroni and Tekke in Cyprus as early as LM II, and Mycenaean II A from Egypt, Lachish, Ras Shamra, and Enkomi in Cyprus. A fine vase from Aniba in Nubia may be LM I A or B, but may be a Syrian imitation of Minoan. But it is only Myc. II A ware that becomes frequent, about 1450.

With this is obviously connected the old question 'Who were the *Keftiu* who bring tribute on Egyptian tomb-paintings about 1500?' Furumark infers from the associated names of peoples, that they came from the mainland north of Syria, perhaps from Cilicia; but his analysis of the style of the frescoes reveals a more and more conventional repertory of personages and offerings, though the earliest, from Senmut's tomb, may have been a real embassy from overseas, with Minoan vessels. Most of the others are Syrian, though more or less Minoanized. He disputes Wainwright's reliance on the historical accuracy of the painters, while accepting many of his arguments.

The consequent reconstruction of economic and political history may almost be anticipated. It is most carefully worked out and a valuable contribution.

ERIC J. HOLMBERG. 'Some Notes about the Ethnical Relations of Prehistoric Greece'. pp. 129-38.

Starting from Blegen's 'Athens and the Early Age of Greece' in *Harvard Studies No. 5*, this careful paper distinguishes the principal archaeological elements in early Greece and the Aegean, and tries to assign them to diverse intruders. The writer accepts a primitive neolithic population, continuous with that of Anatolia, and in contact, perhaps by sea, with Cilicia or Syria; whence it may have drawn its first use of pot-painting, in 'Chaeronea ware'. He admits great difficulties, but over-estimates that of distance, in view of the ease of coastwise navigation with an *imbat* daily breeze. He thinks the Thessalian 'carboniferous ware' may be Danubian, and looks to the Ukraine and Bulgaria for the boldly painted ware of Dimini. About Crete he is over-cautious, and he seems to

overlook the incised 'Pelos'-ware of the Cyclades, with its Libyan resemblances and wonderful sketches of seagoing ships with ensigns. All this he puts earlier than the arrival of Anatolian folk with spout-vases, copper, and (as Dr Holmberg suggests) the so-called 'Anatolian' terminations of place-names. He does not discuss the Minoan and Cycladic painted ornament, with its spirals and naturalism; and he thinks the South Aegean got its copper from Cyprus, though the types of implement point rather to Anatolia, and there is a great gulf between Crete and Cyprus till much later.

Finally, another East European invasion, illustrated by the Sixth town of Hissarlik is credited with the spread of Indo-European speech in the Aegean, and the great devastations of the Middle Bronze Age. Here the story rather fades out, for there is a good deal more to say about the local struggles of Aegean people, even before the end of the Bronze Age.

JOHN L. MYRES.

A TENTATIVE STUDY OF THE PLEISTOCENE CLIMATIC CHANGES AND STONE AGE CULTURE SEQUENCE IN NORTH EAST ANGOLA. *By* L. S. B. LEAKEY, *Museu do Dundo*. (Lisbon, 1949).

Previous reports from the energetic and astute diamond-fields geologist Mons. J. Janmart, from north-eastern Angola have made it very clear that this is a rich and important field of African prehistory. The dating of diamantiferous gravels is of economic importance here as on the Vaal River. Certain gravels are devoid of diamonds; if these can be recognized by their tool or fossil content, considerable labour and expense can be saved. It must constantly be remembered that we are dealing with an area less than 8°S. of the Equator, an area in which to-day there is a mean annual rainfall of almost 60 inches. It was therefore natural that Janmart should turn to a prehistorian from the tropical belt of Africa, and the present paper consists of a most happy collaboration between Dr and Mrs L. S. B. Leakey and Mons. Janmart.

No more than a brief *résumé* of the sequence is necessary here, and the paper should be read carefully in relation to other recent publications from the Central African field. Following high-level pre-human gravels of an ancient Miocene river system, a series of six climatic episodes, each with fairly well associated tools, covers the Kafuan pebble-culture and the Chelles-Acheul developments. The eighth episode is clearly the most important, it is the Period of Kalahari Formation, possibly extremely dry even in the tropics. It starts with Proto-Sangoan tools, followed by a lower and a Middle phase. Six subsequent climatic episodes formed the post-Kalahari deposits, beginning with the Upper Sangoan tools, leading first to the Final Sangoan, then to the partly derived Lupemban culture and finally to advanced arrow-points, backed-blades, microliths and bored stones. Some of these last are probably to be associated with the polishing grooves for stone axes, the products of which have not yet been found.

Cultural evidence for both the earlier phases on Man's story and the later stages is here weak, but this is more than compensated by the abundant series of Sangoan and Lupemban tools found in relation to geological deposits. Here for the first time truly representative collections have been illustrated and described, and (it is sincerely to be hoped) the terminology fixed for some considerable time to come. These descriptions alone make the work an essential reference; a volume to stand beside Leakey's own work on Kenya, and the more recent books by Neville Jones and by J. Desmond Clark on the Rhodesias. All use the same geological approach to prehistory, and the basic 'Pattern of Prehistoric Africa' is beginning to emerge.

It is evident that the climatic symmetry of Africa (that which is global and unalterable) must be considered to a far greater extent. Pluvial and interpluvial periods may

either have consisted of an expansion and contraction of the existing pattern—an absolute difference—or of an increase or decrease of the swing of climatic belts—a distributional difference. It is here made clear that at 8°S. even in the 'very dry' phase of the deposition of Kalahari Sands, the rainfall was still relatively high and streams ran with a flow sufficient to clear the beds of local streams while locally man appears to have survived and been fairly happy. Laterites, needing a mean annual rainfall of 40 inches, low cloud-cover and high evaporation for part of the year, were able to form.

In terms of the rest of Africa, the western equatorial region must necessarily have always had a high rainfall. Eastern equatorial Africa must conversely have had a relatively low rainfall. We must therefore in future consider only two appropriate climatic regions within the African tropics. The western region of high rainfall has consistently yielded the advancing Sangoan adze culture, presumably a wood-working development; while the dryer region to the east has just as consistently yielded point-burin-blade cultures, a typical hunting complex. At different times Leakey has shown how each of these divergent evolutions has its roots in the African Chelles-Acheul series, each may thus be a regional adaptation of the older unspecialised cultures through the accentuation of bifaced and unifaced elements.

The publication under review carries the unavoidable implications that the Victoria Nyanza Basin falls naturally into the region of higher rainfall, that Olduvai is a most crucial site and that everything east of that gorge and the Mau Hills belongs to the dryer region of the east. The falling off of the Sangoan complex as we pass southward out of the tropical belt has been clearly shown by J. Desmond Clark. Here a series of variants arise in direct relationship to decreasing rainfall, finally petering out in Neville Jones' Bembesi Variant in Southern Rhodesia. The general introduction of the blade and point forms with the Lupemban, as the major pluvials disappear, is evident within the tropical belt.

Great advances have been made in Central Africa in the past two years. We must expect a period of digestion and contemplation before the immense progress can be assimilated, either in Africa or in Europe; but it is most devoutly to be hoped that no further experiments in the basic terminology of Africa (either climatic or cultural) will be attempted until the way is clearer. Leakey has firmly taken the terminology of the Sangoan in hand; but unhappily he has added a new climatic term (Kanjera) for the Upper Kamasian Pluvial, a term hardly justifiable either here or elsewhere on purely climatic grounds, though it conveniently limits the Middle Pleistocene.

A. J. H. GOODWIN.

FRANCISCAN AWATОВI. THE EXCAVATION AND CONJECTURAL RECONSTRUCTION OF A 17TH CENTURY SPANISH MISSION ESTABLISHMENT AT A HOPI INDIAN TOWN IN NORTH-EASTERN ARIZONA. By ROSS GORDON MONTGOMERY, WATSON SMITH and JOHN OTIS BREW. *Peabody Museum Papers*, vol. XXXVI, Cambridge, Mass., 1949, xxiv+361 pages, with plates, plans and drawings. Price \$5.85 (paper), \$8.35 (cloth).

This report immediately calls to mind Kenneth E. Kidd's report on *The Excavation of Ste Marie I* which was reviewed in the September (1950) number of *ANTIQUITY*. Both are concerned with abandoned 17th century mission sites in America, and both demonstrate with welcome emphasis the potential archaeological contribution to modern history. The Spanish Franciscan site at Awatovi in Arizona is more spectacular than the French Jesuit site at Ste Marie I in Ontario, and the Peabody expedition, under the able direction of John Otis Brew, was more lavish in conception and execution



than Kenneth E. Kidd's expedition. The present report, therefore, is more ambitious in scale and scope. But as a record of an archaeological operation and as a model of publication it is not superior to its modest Canadian counterpart. Indeed it is perhaps too lavish of printer's ink. It is almost wastefully reminiscent and leisurely in its style—at least to eyes in hurried and austere Europe. The text could be cut considerably with gains in lucidity and without loss of vital information. And more and better plans and sections would have been an advantage.

There is a long chapter, the longest in the report, in which a scholarly architect attempts 'a conjectural reconstruction of the life of 17th century Spanish Awatovi as well as of its buildings'. This is assuredly a most interesting and valuable section, and it is defended as 'a new technique which modern research is providing'. But it is unwise to hasten the development of a new technique or to submit it to strain in its infancy, and it would have been better, perhaps, to have published this section separately. Despite valiant attempts at co-ordination it still sits awkwardly in an archaeological report. It is brilliant in itself and it is significant as a pointer to the future, but as part of 'a new technique' it is not quite ready for display to the world of scholarship.

F. T. WAINWRIGHT.

TRELLEBORG. *Af* POUL NØRLUND. *Nordiske Fortidsminder, København, 1948.*  
296 pages plus illustrations. No price.

If Danish archaeologists had given the world nothing but this single large volume their reputation would be deservedly high. *Trelleborg* is beautifully produced and lavishly illustrated, and the material within its (paper) covers is worthy of its setting. It is the report on excavations carried out between 1934 and 1942 by Dr Poul Nørlund at Trelleborg in West Zealand, the fulfilment, we understand, of a youthful ambition.

The circular fortress or *ringborg* at Trelleborg is now revealed as the headquarters of a warrior community, a kind of army training centre, planned and constructed with a precision which leads the author to look for Roman influence. Dr Nørlund describes it as a 'sjællandsk Jomsborg', an apt description, for his work has proved that such places as Jomsborg did exist, places 'where young men were trained under discipline and stern regulations for the practice of war and for rigorous voyages over the seas'. Similar sites are known, and in 1945 excavation was begun at Aggersborg, another and perhaps larger fortress of the Trelleborg class.

Undoubtedly this report is of first-rate importance to all students of the Viking period. Its interest for English students is increased by the fact that Trelleborg may have been one of the centres from which King Swein Forkbeard launched his attacks on England. Danish fortresses, especially of an earlier age, are known from historical sources to have existed in England, but none have been identified on the ground. Dr Nørlund's work will be indispensable to those scholars who are searching for Danish fortresses in England, and he offers clues which should be followed up.

The credit for this masterly excavation belongs to Dr Nørlund. But he has gathered together a team of specialists, and they have written sections of the report dealing with such topics as the geological features of the site, pre-Viking settlements, etc. Dr Gunnar Knudsen, the Danish place-name scholar, has written twenty-five pages on 'The Name Trælleborg'. This is an indication of a scale of publication lavish beyond the dreams of British scholars.

It is impossible to do justice to this work within the limits of a short review. Adequate treatment would require a whole number of *ANTIQUITY*; that would perhaps give some idea of its importance for Trelleborg is in its own way as significant for Dark

Age studies as Sutton Hoo. One day we shall find an English 'Trelleborg'. May we also find an excavator of Dr Nørlund's calibre and the funds to produce a report that will bear comparison with his own impressive volume.

F. T. WAINWRIGHT.

**TROY: GENERAL INTRODUCTION; THE FIRST AND SECOND SETTLEMENTS**, by CARL W. BLEGEN, JOHN L. CASKEY, MARION RAWSON, JEROME SPERLING. *Two parts*, xxvii+396 pp., text, 471 figures. *Princetown University Press* (London, Geoffrey Cumberlege). 1950. 235s.

Troy was the first tell to be excavated stratigraphically anywhere in the world. However grave his technical shortcomings, Schliemann deserves full credit for recognizing in 1871 that he was digging a mound formed by the ruins of seven superimposed settlements. Subsequent excavations and Dörpfeld's assistance of course increased the number of 'Troys' to nine and modified Schliemann's conclusions on many points, but the skeleton of the culture-sequence and the richest items of its content remain Schliemann's. In the sequel, close analogies to Trojan bronze ornaments, stone battle-axes and bossed bone plaques in Central Europe and the Western Mediterranean showed Troy to be a key site for the prehistoric chronology of our continent, while its sequence of stratified relics should have provided a standard of comparison for the 'pre-Mycenaean' cultures that began to turn up in the Aegean from 1894 on. Instead the Minoan and Helladic series, dated by Egyptian contacts, came themselves to form a chronological frame into which the sequence of 'Troy' should be fitted. Unfortunately, while Helladic sherds could be recognized at the site, their stratigraphical context proved to be indeterminable, and the exact position of the objects most significant for European chronology was found to be questionable too. It was in the hope of resolving these ambiguities, and also of obtaining further data on the oldest culture of the site, that the University of Cincinnati Expedition began the re-excavation of Troy in 1932. Work was continued every year till 1938, and short reports on each season's work appeared annually in *A.J.A.* so that the main certain results of the excavation are already known.

But the definitive publication, embodying the results of an exhaustive analysis of the gigantic masses of pottery and other relics and presenting the evidence for the excavators' conclusions, has only begun to appear in 1950. The present volume is to be followed by three more dealing respectively with Settlements III-V, VI, and VII-IX and an index volume together with a series of supplementary monographs; the latter will include the report on Kum Tepe which is anxiously awaited since the site seems to be materially older than Troy I. But since the end of the excavations Dr Claude Schaeffer has exposed further contradictions in the reports of Schliemann and Dörpfeld and suggested drastic solutions. His arguments have not been considered in Vol. I, but will probably have to be faced in Vol. II.

The General Introduction, pp. 1-30, besides historical data gives the general classification for metal pins, idols, bone implements and whorls to be followed in the sequel and illustrates the ceramic types, common to all phases of the Early Bronze Age, Troy I-V. Thereafter the account of each period of settlement begins with a general survey of the architecture, distinctive types of pottery and other relics and foreign relations and explaining the subdivision of each period into subperiods or phases; three main phases are distinguished in Troy I, seven in Troy II. Then detailed reports of the excavations in each of the several areas examined are given consecutively for every subperiod. All individual objects are described and discussed in the excavation report devoted to the area and sub-period in which they were found. An exhaustive index of 54 columns, together with an 'Index of References in Part I to Illustrations' at the

beginning of Part II, should enable the reader with a little ingenuity to locate the description of any particular object, technique or structure.

Schliemann had happily left a relatively large area of Troy I intact so the present volume can give a comparatively full account of the culture represented therein. In one area no less than ten architectural horizons and a superincumbent layer of debris (I k) marking the destruction of Troy I could be distinguished; but to provide a general scheme the ten layers are distributed between three subperiods—Early, Middle and Late. The first town wall, of stone, was built in the Middle phase to which presumably belongs also the celebrated sculptured stele. Its resemblances to carvings ‘found in a Neolithic context in caves (*sic*!) in the Marne region and in southern France’ are justly described as ‘rather close to be merely fortuitous’. It was found set on its side and obviously reused, together with two slabs bearing ‘cupmarks’, to fence off a space at the foot of the rampart. Already in Middle and also in Late I a few foreign vases were imported. The Helladic sherds include not only *urfirmis* but also three fragments of patterned ware. In Mainland Greece the latter comes late in the Helladic sequence, but it may be earlier on the islands.

To the same subperiods are assigned ‘bowls’ (A 32) and jugs (B 2) with ‘elbow handles’ (indexed ‘Pottery; structural details; handles; elbow’). Judging by rather inadequate pictures, these seem to represent a curious type known on the one hand in the ‘Chalcolithic’ of Mersin in Cilicia and on the other in Central Europe from the Baden phase on. Finally a broken clay mould for a dagger with a stout midrib proves advanced metallurgy for Middle I and extends the range of the type in the Aegean.

Less was left of Troy II and the expedition had to rely on observations on seven distinct and small sections. The combination of these yields a distinctly lumpy citadel, different at once from Dörpfeld’s ‘Troy II’ and from Schaeffer’s Troy II plus III. To Dörpfeld’s three phases, a, b, and c, there are added four more, d, e, f, and g, completely preserved only in an ‘island,’ some 12 m. square, left standing on the top of the II a rampart terrace just 8 m. south of the great Megaron. At its base on the top of the great rampart and northwards to the Megaron the ground level of Troy IIa lay  $30 \pm .50$  m. above tide mark, but further north in two sections this level was found, resting on I k debris, at 27.20 and 26.50. In both these sections and two others burnt material on the II a, or under the II b, floors might be taken to indicate a violent end to Troy II a (pp. 247, 251, 252, 255).

Prof. Blegen and his collaborators agree with Dörpfeld in attributing to II c the great Megaron with the Propylon and courtyard wall as well as Megaron II f and house II R, and they discovered an extension of the courtyard wall, indeed a colonnade, under the ‘island’. House II R and another building may have been destroyed by fire.

In subperiod II d the major buildings remained standing, but the courtyard was enlarged, utilizing some of the space made available by the final extension of the ramparts in II c. Traces of fire were observed in two sections. Then in phase II f, the ‘island’ area was occupied by houses grouped along a lane to make room for which the courtyard wall must have been demolished. Finally in subperiod II g these houses with floors at about 31.50 extended at least to the edge of the island and to within 4 m. of the Propylon. The excavators believe that the Megaron at least was still standing—it must have been seriously cramped—but on the strength of the style of masonry assign to II g ‘the House of the City King’ where Schliemann found several Treasures. That the Treasures did belong to Troy II g seems to have been established by the discovery of two small hoards comprising typical beads and ornaments in houses of that subperiod. The name ‘Burnt City’ could be justified by layers of burnt debris on II g floors in three of the four areas where the subperiod survived.



But plainly this 'Troy II g' is not Dörpfeld's 'Troy II c', several conspicuous landmarks in which had been replaced by the houses which had encroached on, even if they had not actually invaded, the precincts of the Megaron and the Propylon. It is in fact something substantially later than the old Troy II. Yet it is not Schaeffer's Troy III. That succeeded the Burnt City but comprised, besides the Megaron (the omission of the Propylon from his plan may be an oversight) and the Treasures, a huge brick rampart, 'le plus formidable de Troie'. This has been encountered in no section here described but the length described by Dörpfeld is assigned to II d; on Schaeffer's plan it would have run right through 'the House of the City King' and therefore be later than Troy II g.

Of course the archaeologists of the Cincinnati Expedition have defined their sub-periods as much by ceramic as by architectural evidence. But they have to admit that they did not recover much well certified material, at least for phases II a to II d; for the intact island yielded practically nothing anterior to II e. Fundamentally the pottery of Troy II carries on the traditions of I, but some new techniques and shapes emerge and old shapes, like the elbow-handled jugs, went out of fashion. An event of sociological and demographic, as well as technical, importance should be the 'introduction of the potters' wheel' which means the establishment of professional potters producing *en masse* for a local market. But in the book no prominence is given to the event which on the strength of a sherd found under a dated wall in the island is assigned to subperiod II b (p. 255); wheel made ware was in any case plentiful by II c. A new form, once taken as distinctive of 'Troy II', is the misnamed 'δέπας ἀμφικύπελλον'. The recent excavations have shown that it was also manufactured in abundance in Troy III and IV and occasionally in V. Fragments from two reliable II c layers, under the island and below 'the House of the City King' respectively, should resolve Schaeffer's doubts as to its currency in Troy II; numerous specimens were recovered from the more prolific later layers of II. Face urns are represented first in II g, but one face lid is reported from phase II d.

How far then do the excavations published here elucidate the problems they were undertaken to answer? The stratigraphically established Trojan sequence has in fact been correlated in a general way with the Early Helladic. But the precise correlations are, as has already been hinted, a little ambiguous, and in any case Helladic chronology is itself too uncertain to provide absolute dates for the periods here discussed. On the contrary, the deep deposits and many building phases containing Early Helladic imports at Troy do afford presumptive arguments for a high chronology for the whole Aegean Early Bronze Age. Types of battle axe and metal weapon invoked for comparison with cis-Balkan specimens have been pushed back into Troy I without thereby defining any better their significance for European prehistory. The position of the Treasures in subperiod II g of the local sequence has been settled without thereby defining more precisely their age within the third millennium. From the same horizon the excavators recovered a bossed bone plaque, but, to judge by the photograph (which may be misleading), it differs so seriously from Schliemann's specimens that one hesitates to accept it as dating the latter and their Sicilian and Maltese analogues. The answer to Schaeffer's questions must await volume II.

We must be grateful to Prof. Blegen and his colleagues for presenting so fully and with such a wealth of illustration the evidence on which their conclusions are based. But just because this is the first volume of a series, it may be useful to mention some improvements which seem desirable. The authors are aware that stone implements are potentially significant, but seem unaware of what has been written about their manufacture, their use and their classification in Danish, English, French and German. Thus for



'celts' they use the rather old-fashioned classification adopted by Tsountas in 1908 without even giving, as he did, longitudinal sections. Flint and obsidian implements are illustrated only by photographs which are useless, though of course drawings, if executed by a draughtsman not initiated into the universally approved conventions, would be positively misleading. In any case 'a pointed implement' of flint in which 'two small holes appear in break at blunt end probably for attachment of handle or shaft' sounds so unusual as to deserve a good drawing. Excavators in the Near East are usually careful to give the size of bricks encountered. Here this is done only once and no attention is given to the question as to whether the bricks were formed in a mould or by hand.

V. G. CHILDE.

**UAXACTUN GUATEMALA: EXCAVATIONS OF 1931-1937.** By A. LEDYARD SMITH. 93 pages + frontispiece and 143 Plates, maps and diagrams. *Carnegie Institution of Washington*; publication no. 588. Washington, D.C. 1950. \$9.00.

Uaxactun, in the heart of the densely forested Peten District of Guatemala, is near the centre of the Maya area, and like so many Maya cities consists of numerous complicated clusters of courts, temples and pyramids. Though considerably smaller than the neighbouring city of Tikal (which has never been properly excavated), it must have been a site of great importance throughout the so-called Old Empire, or Classic period.

The earliest known dated Maya Monument was found here, and other inscriptions show that it was occupied continuously from about A.D. 430 to about A.D. 970. Excavations carried out by the Carnegie Institution from 1926 to 1931 in the complex known as Group E, and described by O. G. Ricketson showed that the 'city' had been occupied long before the earliest dated stelae, and that even then the Maya built pyramids.

The later excavations, described in this work, were chiefly concerned with two complexes known as A-V and B, although the whole site was completely remapped by E. M. Shook, and many minor excavations were undertaken. The growth of structure A-V could be traced from three simple temples on a low raised platform in the pre-classic phase through successive rebuildings until it was abandoned at the end of the so-called Old Empire. At this time it boasted four courts and numerous palace-like buildings which completely engulfed the three original buildings. It was thus possible to establish a complete sequence of architectural styles of some seven hundred and fifty odd years together with associated masonry construction and pottery types.

In Group B was a most interesting wall-painting, below which was a painted inscription showing part of a ritual kalendar, described by Eric Thompson, in a technical note, as being different in form from those in the codices, and dating from a time before the written glyph shapes had been properly formulated.

Unfortunately the preparation of this report was delayed by the war, and it was decided to publish reports on the pottery and other artifacts separately.

This volume is thus largely restricted to the architectural features and detailed accounts of stratification and burials, with comparatively little discussion of the smaller artifacts. Therefore the reader must refer to *The Artefacts of Uaxactun*, by A. V. Kidder, for a critical study of the non-pottery material and the evidence of far-reaching trade which they provide, and to a forthcoming report on the pottery by R. E. Smith. When this is completed all the results of what is possibly the most important excavation ever undertaken in the Maya area will be available. Even by itself this work is of great value and interest. The introduction by A. V. Kidder, is as we would expect a scholarly summary of Maya history in relation to Uaxactun, and a series of drawings by Miss



Tatiana Proskoriakoff show graphically, as no diagram or written word could, the principal phases of the growth of Group A-V.

Mr Smith who was in charge of the excavations must be congratulated on his results and on the report which is characterized by the thoroughness which is typical of the work of the Carnegie Institution.

ADRIAN DIGBY.

**ARCHAEOLOGICAL MAP OF THE HASHEMITE KINGDOM OF THE JORDAN:** Scale 1 : 250,000 : compiled and drawn by the Department of Lands and Surveys of the Jordan, from information supplied by the Department of Antiquities, 1949 : *printed by the Ordnance Survey, 1950, and sold by EDWARD STANFORD LTD., 12-14 Longacre, London, W.C. 2 : three sheets, Amman, Karak, Ma'an.* Price not stated, but ascertained to be 3 shillings for each sheet.

Archaeological and historical maps are of two kinds—those which mark the sites of ancient remains of all periods on a single sheet, and those which adopt the principle of 'one map, one period'. Maps of the first kind, to which those under review here belong, are useful as index-maps for many purposes. They serve the practical needs of the archaeological administrator and field-worker, and they will be found very useful by all students of the history and archaeology of the country. But they should be regarded as achieving a limited objective, the ultimate objective being a series of period-maps of the second kind, not necessarily on the same scale. These three maps are of the greatest value because they show for the first time, accurately, and on a reasonably large scale, the position and distribution of a great number of ancient sites, most of which have never before appeared on anything that could be called a map at all. Those responsible are to be congratulated on the successful achievement of a limited objective.

The physical basis is a good conventional system of contours and layer colouring, with certain special symbols demanded by such natural peculiarities as mud-flats, lava and wadi-spreads. Ancient remains are rightly classified chronologically and indicated by symbols. The first of nine such periods is called 'prehistoric' and the symbol (which looks like an egg) is no doubt a hand-axe. The range covered is given as c. 12,000-4000 B.C., which is perhaps the best that can be done in the present state of knowledge. The Chalcolithic Period (c. 4000-3000 B.C.) is indicated by a dolmen, the Bronze Age (c. 3000-1200 B.C.) by a globular pot, Roman remains by fasces and Arab by a crescent. An inset occupying a blank area calls attention to 'sites of special interest'. This is quite a good idea, but there is room for some improvement in details, and a bad slip of a century in the date of Qasr 'Amr (about A.D. 714 not A.D. 614), and the description might be amplified by a mention of the bath-buildings on which the frescoes are drawn. The description of the port at the head of the Gulf of Akaba as 'Etzion Geber, Solomon's famous fort, also a large copper smelting center' also appears to have some curious spellings, and 'fort' must surely be an error for 'port'?

The maps are published as flat paper sheets. No doubt that is concession to the needs of today, but it entails inconveniences of handling and storage. The proper format for all such maps is that adopted for its period maps by the Ordnance Survey—folded in a cover, with an accompanying explanatory text. Such a text is necessary to amplify the short descriptions which for reasons of space must leave much unsaid; we should like, for instance, to be told more about the evidence for 'uncertain' Roman roads; is it the existence, the age or the exact course which is uncertain? We are also uneasy about that chalcolithic dolmen-symbol, and about the use of that word 'dolmen' which has caused so much trouble in the past. The megalithic burial-chambers of Jordan are often



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associated with nuraghi-like buildings that are presumably much later than 3000 B.C. and sufficiently distinctive to deserve a special symbol.

But all these are minor points not seriously affecting the utility of the maps and easily capable of amendment (if necessary) on a second edition. One must remember that the information herein embodied is the work, not of a large number of people spread out over a long period and subjected to constant tests by other workers (as in the older lands of Europe), but of a very few people for a very few decades. All the more honour to them for having done so much. O.G.S.C.

### SOME APPLICATIONS OF STATISTICS TO ARCHAEOLOGY. By OLIVER H.

MYERS. *Government Press, Cairo, for the Service des Antiquités de l'Égypte, 1950. Paper covers, VI+37 pp. with 19 figures. 14½×11 inches. Price P.T. 120 (about 25 shillings).*

Mr Myers is rightly convinced that 'if statistics be fully applied to archaeology it will be possible not only to draw new knowledge from the clues we now possess, but to obtain information from sources which have never been considered as evidence'. The volume under review contains four essays, the fruit of much labour, dealing with the application of statistical methods of correlation to data collected during excavations in Egypt, Aden and the Sudan over a number of years—data collected in the main without any knowledge of what would be required. They are demonstrations of the use of the statistical tool rather than records of its successful application to the solution of specific archaeological problems insoluble by other methods.

The most valuable of these essays is the longest and last, and is concerned with the statistical examination of surface sites on which potsherds and other objects of widely differing periods may be found together at the same level. It is here shown how, by calculation allied with graphs of various kinds, purely surface sites can be fixed, chronologically or at least culturally, with the same certainty as stratified sites. As Mr Myers himself points out, the greatest possibility for the application of this technique would seem to be in the north African desert.

The other essays deal with the relationship of stone implements and pottery in a stratified site; the sizes of sherds from desert sites; and the hardness of ancient pottery. In the course of this last the author makes the interesting observation that wares increase in hardness after exposure for long periods on the desert surface, and 'it seems possible that this hardening may be brought about by a deposition of silica similar to that from corn stalks on flint sickles'.

The book is severely technical in character and assumes in the reader a knowledge of statistical terminology or a willingness to acquire it from text-books. Mr Myers does not underrate the extra burden thrown on the field worker in collecting data suitable for statistical analysis, nor does he suggest that statistical methods can be indiscriminately applied to all excavations. Enthusiasm does not distort his judgment. The archaeologist starting a new excavation is advised to consult a statistician at an early stage of the work to find out whether or not statistical methods can profitably be used. If the answer is encouraging the necessary technique can easily be learnt.

It is probably not Mr Myers' fault but his misfortune that so little has been done by the Egyptian Government Press to attract the reader who may be interested in a subject likely to become of increasing importance. No indication is given in the book as to how and where it can be obtained in these days of currency and other restrictions, and the price (not printed) seems high for a not very lengthy publication with no binding case. And was it really necessary to adopt so large and awkward a format? F. ADDISON.